THURSDAY, DECEMBER 11, 1890.

A CURE FOR TETANUS AND DIPHTHERIA.

THE greatest interest has been aroused in scientific circles in Berlin by a paper in the current number of the *Deutsche medicinische Wochenschrift*[1] by Behring and Kitasato. These well-known bacteriologists, who for a long time past have been working in Dr. Koch's Hygienisches Institut, have not only succeeded in producing immunity against diphtheria and tetanus, but also in curing animals already infected by these diseases. Their results are to a great extent self-explanatory, and there is every reason to expect that the same method will be found to be applicable to other infectious diseases. The most remarkable part of their discovery is the fact that the blood of an animal that has been made immune against diphtheria possesses the extraordinary power of destroying the poison formed by the microbe of this disease. This power is also possessed by the serum of such an immune animal, which serum can therefore be used as a curative means on other animals that are suffering from this disease. The same statement holds good for tetanus.

Before describing in detail these interesting results, it will be well to give a short historical review of some recent bacteriological work which can be regarded as having led up to this discovery.

Towards the end of 1888 Nuttall,[2] working in Flugge's laboratory at Breslau, discovered that various bacteria are destroyed when mixed with fresh blood or blood-serum, and further that this destruction cannot be ascribed to the action of cellular elements, but rather to the fluid part of the blood. This discovery (which really arose from the German criticism of Metschnikoff's phagocyte theory) was soon followed by the work of Buchner[3] and Nissen[4] on the bacteria-killing power of the cell-free blood-serum. These authors considered that their work necessitated a limitation of the phagocyte theory, and suggested a new view of the nature of immunity, whether natural or acquired. In other words, they suggested that immunity was conditioned by the bacteria-killing power of the various body fluids rather than by that of any particular kind of cell. These opinions were rather severely criticised in a paper by Lubarsch[5] that was published towards the end of last year. Lubarsch emphasized the fact that while the serum of the rabbit—an animal extremely sensitive to anthrax, has a great power of destroying anthrax-bacilli, horses' serum has no such power, although this animal is comparatively refractory to the disease. Again, while on the one hand such eminently pathogenic microbes as the anthrax and cholera bacilli are capable of being destroyed by serum from various animals, several perfectly harmless microbes find blood-serum to be an excellent food-medium. Further, though the serum of the rabbit kills anthrax bacilli in a preeminent degree, the living blood-plasma of this animal can only do so to an infinitesimal extent. Such considerations suggested to Lubarsch that the bacteria-killing power of the blood-serum was a fact rather of the nature of an epi-phenomenon than an essential factor in the conflict between the organism and the microbe. In May of this year appeared my own work on "Defensive Proteids."[1] I gave this name to a new class of proteid bodies, which I found to possess a bacteria-killing power, and which I have obtained from the spleens and lymphatic glands of various animals. This work has a distinct bearing on the foregoing, in that it suggests that the bacteria-killing power of blood-serum is due to minute traces of these substances liberated from the breaking down of lymphatic cells. The absence of a bacteria-killing power from certain kinds of serum (*e.g.* horse) and from living blood-plasma (as has been shown for that of the rabbit in regard to anthrax), appears to be connected with the intactness of the leucocytes in these special cases. Further, the fact that I obtained these substances from cells which either are, or can become, phagocytes, may be taken as an additional proof of Metschnikoff's well-known theory. These substances appear to be absent from the normal blood-plasma, or, at any rate, only present in such small quantities that they cannot be separated from it. With blood of febrile animals, however, the case is different, and from such blood I have been able to isolate a bacteria-killing substance.[2] This fact appears to indicate that these substances are actually used by the organism in its reaction against the attack of pathogenic microbes.

During last summer, while I have been engaged in this work, various other papers have appeared, which tend to show still more clearly that the bacteria-killing power of the blood serum (or if my work be accepted, of defensive proteids) is of real importance in the production of immunity. Bouchard[3] was, I think, the first of many authors who have succeeded in showing that the bacteria-destroying power of blood-serum from immune animals, is greater than that of normal serum. Bouchard proved this in the case of bacillus pyocyaneus for rabbits. He made the animals immune by injections of sterilized culture fluids, and found that serum from such animals exerted a far greater "bactericidal" action on the microbe in question than serum from a normal animal. Behring and Nissen,[4] in a paper published in May of this year, went a step further. They showed that whereas blood serum from an animal made immune against anthrax exerted an increased bactericidal action on the anthrax bacillus, it showed no increased action on the bacillus pyocyaneus. Conversely blood-serum from an animal made immune against the latter microbe, had no increased action on the anthrax bacillus, though it exerted a powerful bacteria-killing action on pyocyaneus. The authors considered that they had proved the ex-

[1] No. 49, December 4, 1890, p. 1113. "Ueber das Zustandekommen der Diphtherie-Immunität und der Tetanus-Immunität bei Thieren."

[2] "Experimente über die bakterienfeindlichen Einflüsse des thierischen Körpers" (*Zeitschrift für Hygiene*, vol. iv. p. 353).

[3] "Ueber die bakterientödtende Wirkung des zellenfreien Blutserums" (*Centralblatt für Bakteriologie*, vol. v. p. 817, and vol. vi. p. 1).

[4] "Zur Kenntniss der bakterienvernichtenden Eigenschaft des Blutes" (*Zeitschrift für Hygiene*, vol. vi. p. 487).

[5] "Ueber die bakterienvernichtenden Eigenschaften des Blutes und ihre Beziehungen zur Immunität" (*Centralblatt für Bakteriologie*, vol. vi. p. 528).

[1] "A Bacteria-killing Globulin" (Proceedings of the Royal Society of London, vol. xlviii. p. 93), and "The Conflict between the Organism and the Microbe"; Part 2, "On Defensive Proteids" (*British Medical Journal*, July 12, 1890.)

[2] "Indications of a Method of Curing Infectious Diseases." Read at the Leeds Meeting of the British Association for the Advancement of Science, September 1890.

[3] "Sur l'effet des produits sécrétées par les microbes pathogènes" (Paris, 1890).

[4] "Ueber den bakterienfeindlichen Einfluss von verschiedenen Serumarten" (*Zeitschrift für Hygiene*, vol. viii. p. 412).

istence of two bodies, each having a specific action on one of the two microbes in question; and further that these substances are present in animals made immune against the above-named diseases. These remarkable conclusions acquire a still greater interest when received in the light of a research by Gamaleia published at the beginning of last year.[1] Gamaleia found that the aqueous humour of a sheep, about three days after inoculation with attenuated anthrax, acquires bactericidal properties for this microbe. This condition lasts for nearly a month, and then gradually vanishes, though, as is well known, the sheep remains immune for a far longer period. These researches, therefore, suggest, firstly, that when an animal has been made immune against a pathogenic microbe, its blood and other body fluids contain a substance capable of destroying the microbe in question; secondly, it follows that such protective substances can remain in the body undestroyed for a considerable time; and thirdly, that they can be present in such quantities as to be able to kill the microbes involved (even without the help of living cells) and yet produce no appreciable ill effect on the general health of the animal. If this is so, why should it not be possible to cure any infectious disease by injecting a "lymph" obtained from the blood or tissues of an animal previously made refractory to the disease in question?

Whether or not the above considerations stimulated the researches of Behring and Kitasato, their work affords a positive answer to this question, which promises to be of the greatest importance to humanity, and has led them to the most unexpected and interesting results from the scientific standpoint. The following is a summary of their paper, which is of the nature of a preliminary communication.[2] The method by which, in the first case, they produced immunity against tetanus and diphtheria is not described. Only so much of their results is communicated as is necessary to support the following propositions:—

"The immunity of rabbits and mice against tetanus depends on the power possessed by the fluid part of their blood of rendering harmless the poisonous substances produced by the tetanus bacilli."

This proposition involves a completely new theory of the nature of acquired immunity. Hitherto it has been thought that immunity must depend either on the voracious activity of phagocytes, or on the above-mentioned bacteria-killing power of the blood, or on an acquired tolerance against a poison; and, further, that by the method of residues, any one of these theories could be proved by showing the other two to be false.

Behring, however, was able to prove, by his work on diphtheria, that none of these theories would account for the natural immunity of rats or the artificially-produced immunity of guinea-pigs against this disease. After disproving many speculations on this subject, the above-given explanation was arrived at, but they only obtained a satisfactory proof of its correctness when they began to test it on the tetanus microbe.

Their experiments prove:—

(1) That the blood of rabbits which have been made immune against tetanus can destroy the tetanus poison.

[1] "Sur la Destruction des Microbes dans les Corps des Animaux Fébricitants" (*Annales de l'Institut Pasteur*, 1889, p. 229).

[2] A fuller account will shortly appear in the *Zeitschrift für Hygiene*.

(2) This character can be shown to be possessed by the blood both before and after it has left the vessels, and in the cell-free blood-serum obtained from it.

(3) This character is of so permanent a nature that it is still manifested by such serum after it has been injected into other animals. Consequently, by transfusion of such blood or serum, important therapeutic actions can be obtained.

(4) This power of destroying the tetanus poison is absent from the blood of such animals as are not immune against tetanus; and after such animals have been killed by the tetanus poison, it can be shown to be present in their blood and tissues.

In support of these assertions the following experimental results are brought forward.

A rabbit was made immune against tetanus by a method which will be described in a forthcoming paper by Kitasato in the *Zeitschrift für Hygiene*. To prove the completeness of its immunity, 10 c.c. of a virulent culture was injected into it. Half a cubic centimetre of the same culture was quite sufficient to produce tetanus in a normal rabbit. The treated rabbit, however, remained immune, and it not only showed immunity against the tetanus bacillus, but also against the poison produced by this microbe. For it remained unharmed by an injection of twenty times the quantity of tetanus poison which will kill with certainty a normal rabbit. Blood was taken from the carotid artery of this rabbit. Before clotting occurred 0·2 c.c. of this blood was injected into the body cavity of a mouse, and 0·5 c.c. into that of another. Twenty-four hours later, these mice, together with two control-mice, were inoculated with tetanus of such virulence that the latter showed the symptoms of tetanus after 20 hours, and were dead in 36 hours. Both of the treated mice, on the contrary, remained healthy.

The greater quantity of the blood of this rabbit was allowed to stand, and its serum collected.

Six mice each received 2 c.c. of this serum in the abdominal cavity, and all withstood a subsequent inoculation with tetanus. Control-mice died of tetanus within forty-eight hours.

With this serum the authors succeeded in curing animals that had been previously infected with tetanus. They have also been able to show that this serum possesses an intense power of destroying the tetanus-poison.

Of a ten-days old tetanus culture which had been sterilized by filtering, 0·00005 c.c. was enough to kill a mouse after four to six days, and 0·0001 would always produce the same result in less than two days.

Five c.c. of the serum of a tetanus-immune rabbit was mixed with 1 c.c. of this filtered-culture, and kept for twenty-four hours. Of this mixture four mice each received 0·2 c.c. (that is to say, 0·033 of the original culture, or more than 300 times the quantity which would otherwise be capable of killing a mouse). All these four mice remained in good health. Control-mice, on the contrary, which were at the same time inoculated with 0·0001 c.c. of the original culture, succumbed within thirty-six hours.

All the mice mentioned in each of the above series of experiments have been subjected to repeated injections with the tetanus bacilli, and have shown themselves to be permanently and completely immune.

This result is the more remarkable in that up till now, in spite of innumerable attempts, no one has ever succeeded in making any animal whatever immune against tetanus. A theory of the nature of acquired immunity which at once led to a method of treating the disease which is easy to understand, harmless to the animal, and certain in its effect, must surely possess some basis in fact.

Naturally every kind of control experiment with serum of normal rabbits has been carried out with uniformly negative results. Serum of cattle, horses, and sheep has also been found to have no action on the tetanus poison. The living blood and tissues of an animal which has not been made immune likewise show no power of destroying the tetanus poison, as appears from the following experiment, which has been many times repeated :—

Rabbits into which 0·5 c.c. of a germ-free tetanus culture is injected subcutaneously, succumb after showing typical tetanus symptoms ; almost always a serous transudation is to be found in the thoracic cavity. Of this transudation, 0·3 c.c. is on the average enough to kill a mouse with typical tetanus symptoms. The same is true for the blood.

The authors close their paper by pointing out the possibility that their method of curing tetanus and diphtheria which they have used with such brilliant results on animals so highly susceptible to these diseases as mice and rabbits, may also be used for the far less susceptible hospital patient. They also note the possible influence of their work on the practice of blood-transfusion.

E. H. HANKIN.

THE STEAM-ENGINE.

The Steam-Engine considered as a Thermodynamic Engine. A Treatise on the Thermodynamic Efficiency of Steam-Engines, illustrated by Diagrams, Tables, and Examples from Practice. By James H. Cotterill, M.A., F.R.S., Professor of Applied Mechanics in the Royal Naval College. Second Edition, revised and enlarged. (London : E. and F. N. Spon, 1890.)

IN view of the general interest attaching to the recent improvements in steam-engines, and the consequent gain so remarkably illustrated by the increased speeds of ocean travel, an exposition from a purely scientific point of view of the causes of this progress comes at a very opportune time. The book before us is the second edition of a treatise which was originally published in the year 1877, and has since that time remained one of the chief authorities on the practical applications of thermodynamics.

It seems to be inherent in the industrial development of scientific laws that the course of invention should occasionally lead engineers away from the principles of pure science on which their work is based. The minds of inventors often become so much occupied with the intricate and perplexing problems presented by the mechanical realization of their ideas, that in the specifications of their successful arrangements, the naked principles which are the common and essential part of all practice are sunk far beneath the surface. It is of no ordinary importance that means should exist for co-ordinating the two independent growths of knowledge which are thus indicated. Probably no more weighty or influential calling exists for the industrial progress of the nation than that of the scientific engineers who are able to take into their view both sides of this field, and lead to the fertilization of each by the results of the other.

The progress of the science of heat forms one of the most interesting chapters in scientific history, and illustrates in a remarkable manner the way in which conceptions that at the time of their development appear of the most advanced and recondite character become the common elementary ideas of succeeding generations. The first great advance was no doubt the introduction of precision into the measurement of temperature by the invention of thermometers, and of the method of graduating them by first marking the fixed points on their scales. As Dr. Gamgee has recently pointed out, in a paper read before the Cambridge Philosophical Society, the fixed points by means of which Fahrenheit graduated his original alcohol thermometers were hardly physical at all ; the higher one was the temperature of the armpit or mouth of a healthy man, which he had observed to be constant. It was after the Fahrenheit scale had been fixed by this temperature and that of the best available frigorific solution (snow and salt) that the construction for the first time of a mercury thermometer gave an opportunity of testing the boiling-point of water, which in this way was found to be constant, and under standard conditions to be at the point marked 212° on the scale. It was not until near our own time that the possession of methods of thermometry, combined with the researches of Black and others on the transfer and latency of heat, led, under the influence of Davy, Mayer, and Joule (and also Carnot), to the recognition of heat as a form of energy, and made possible an exact study of its transformations.

The final stage in the dynamical specification of the subject arrived when the very remarkable generalization of Carnot led, in Sir W. Thomson's hands, to a purely physical definition of a temperature scale, and the experiments of Joule and Thomson established the very close approximation of this scale to that of an ordinary thermometer, formed with one of the more permanent gases as expanding substance. This temperature scale enters into the general entropy equation employed by Clausius to express mathematically the results of Carnot's principle ; so that in this form temperature has virtually a definition in analytical dynamics, and is an essential element in any dynamical theory which attempts to explain the phenomena of heat-transformation. These topics, treated in the simpler cases, now form a part of every course of physics.

The entropy formula expresses a relation between the initial and final states of the system, which is independent of the path of transition from the one to the other, provided this path be reversible. It is, therefore, to be classified with the fundamental dynamical investigations of Sir W. Rowan Hamilton, whose principal function is expressed in terms of the initial and final positions of the system, and the energy contained in it. It thus becomes a question of pure dynamics to inquire whether the entropy relation may be postulated as a general property of all motions, or whether its truth is constituted by the

process of averaging the motions which is found essential to the development of the dynamical theory in the cases now in part unravelled, as, for instance, the kinetic theory of gases. In these discussions the chief difficulty is the dynamical realization and interpretation of temperature, as it is defined by its occurrence in the equation of entropy.

The nature of the difficulty is more fully realized when the fact is recognized that in a dynamical system, involving no dissipation of its energy to other systems, an exact reversal of the motions of all its molecular parts would cause it to retrace its original path, and would therefore reverse any degradation of energy in the thermodynamic sense that may have occurred in it. Thus, not merely is it possible for the thermodynamic order of Nature to be reversed by a continuous expenditure of intelligence without expenditure of energy, as in the case of the demons introduced by Clerk Maxwell into the exposition of the theory of gases; but a system may actually be put into such an initial state that it will go on reversing the laws of entropy of its own accord.

It is true that we are saved from the necessity of having to admit that such exceptions to the course of Nature may be actually existent; for all transfers of heat are inextricably mixed up with the phenomena of radiation, which involve transport of energy through the ether with a velocity the same as that of light, and which, therefore, require that if our system is to be absolutely conservative it must include the ether and extend beyond the farthest star. In any portion of a system thus constituted, however, we may suppose degradation staved off and reversed by a complete reversal of its motions, the effect produced will be only temporary, and degradation will ultimately reassert itself.

Nevertheless, the problem remains for solution, how far the law of entropy, whose truth in a limited range is demonstrated by the fact that it is fundamental to the wide and firmly established science of thermodynamics, is to be considered as a principle of a purely dynamical character; and although the investigations of Clausius, von Helmholtz, Boltzmann, J. J. Thomson, and other mathematicians who have attacked the subject, have thrown much light on its affinities to known analytical laws of pure dynamics applied to systems in a steady state of motion, yet it is not too much to hope that there is a great deal more to be gained in this department of investigation.

In particular, a theoretical basis has yet to be supplied to the applications of the law of entropy to purely chemical actions. The results of its application to voltaic phenomena by Willard Gibbs and von Helmholtz have met with satisfactory experimental confirmation; while its extension to the theory of osmotic phenomena, dissociation, and the whole field of the chemical action of dilute solutions seems to be at any rate in qualitative accordance with facts, and even promises to fundamentally modify some of our notions of chemical action.

And there yet remains for answer the question which has long been put, whether there is any reason to suppose that the activities of living animal (or plant) tissue are limited by the principle in the same way as are those of dead matter.

The fundamental character of the views opened up by the development of Carnot's ideas on the efficiency of heat-engines adds a strong theoretical interest to the manner in which engineering practice has approximated to its ideal conclusions. Even though their full and complete application in the domain of heat-engines would not be questioned by anyone, yet it is clear that the investigations that have arisen out of their experimental testing and improvement have much value as illustrations and models for similar inquiries in more abstract departments of physics. Indeed, the subsidiary discussions in this treatise, relating to causes of waste of available energy, remind one, on a magnified scale, of the corresponding corrections necessary in thermo-chemical investigations, particularly those relating to entropy; and there seems to be a considerable field in which the one subject may profit from a study of the other. In this connection the admirable arrangement, discussion, and analysis contained in the chapters on the causes of loss of efficiency supply a store of information and general principles that would not otherwise be easily available for non-professional workers.

It is unfortunate that the science of thermodynamics suffers more than any other department of physics from the difficulty of exact comparison between theory and experiment. It may be a surprise to some people to learn that even the value of the mechanical equivalent of heat is hardly certain within 1 per cent.: it appears that recent experiments by Rowland give a result nearer to 782, that derived by Joule from the electrical method, than to 772, the value finally put forward by him from the results of several concordant series of direct experiments.

The treatise which has suggested these remarks contains a connected account of the different ways in which thermodynamic theory has been applied and realized, the subjects treated and co-ordinated ranging over air-engines, gas-engines, guns or powder-engines, reversed air-engines or refrigerators, and, in greatest detail of course, the different types of steam-engine. The reader will find in it a simplicity in the statement of physical results, and a freedom from the encumbrances of algebraic analysis in the discussion of general laws, which form one of the highest merits of a treatise on the principles of natural philosophy, and the most fitting preliminary to the examination by mathematical analysis of special problems. This is combined with a very interesting account, illustrated by actual examples derived from recent practice, of progress made in actual construction towards the ideal of perfection, which it would be beyond our province to refer to in detail. From the point of view of the student of physics, the book forms a most valuable supplement and corrective to the necessarily abstract discussions which form the substance of treatises on theoretical thermodynamics.

J. LARMOR.

THE CLASSIFICATION OF ANIMALS.

Zoological Types and Classification. By W. E. Fothergill, M.A., B.Sc. (Edinburgh: James Thin, 1890.)

A Zoological Pocket-book or Synopsis of Animal Classification. By Emil Selenka and J. R. A. Davis, B.A. (London: C. Griffin and Co., 1890.)

THE author of the work bearing the first of the above titles has set himself a most formidable and ambitious task, viz. that of compressing into a small volume

of 214 pages diagnoses of the organology of all classes and orders of living animals, together with brief descriptions of the structure of types of the latter, especially where represented by familiar creatures, and of adding thereto a list of most of the leading families, with short diagnoses of the same when occasion should demand. Such a book to be of real service should be as free of errors as possible; and, in view of the unprecedentedly rapid growth of zoological literature within the last two decades, the difficulties of the undertaking might be expected to be inversely proportionate to the smallness of the volume. The author sets out with the Protozoa, and advances, in ascending order, to the Primates. The plan of his work is a good one. Each fasciculus leads off (with few exceptions) with a recapitulation of the characters distinctive of the class with which it deals, and then follows a brief description of some central representative of each of its leading orders, and, in many instances, a tabular *résumé*. The book concludes with some notes upon the maturation and segmentation of the ovum. In dealing with the vertebrate vascular system, the author resorts to the construction of remarkable schemes which he believes may "aid the formation of a mental picture"; they appear to us to very efficiently confuse the mind, and we urgently recommend that their place be taken by diagrams which shall delineate the vessels themselves. The book, regarded as a piece of clerical work, is a good one, and the author has taken immense pains in compiling it. Its utility is, however, seriously marred by the constant recurrence of small errors, with which, as with insufficiently guarded assertions, it teems throughout. The mischief wrought by such, as distinguished from gross errors, which the student is tolerably certain to detect and rectify for himself, is, as the working of a subtle poison, slow but sure, and they cannot be too strenuously guarded against in an elementary treatise. Omissions, and occasional wrongly constructed sentences occur, and there is evident in places a want of uniformity of treatment—as, for example, the summary dismissal of the Echinodermata, and the non-recognition of the families of the important order Insectivora. Important groups, such as the Choano-flagellate Infusoria, and important characters, such as those of the central capsule of the Radiolaria and of the dentition of the Marmosets, pass unnoticed; while absolutely erroneous definitions are given of leading organs, such as the brain of the Craniata and the Arthropod eye—to say nothing of the relegation of the Arachnida and Prototracheata to the class Tracheata. There undoubtedly exists a demand for a book such as this which shall be up to date; and if the author will carefully revise his work this demand will have been met. The volume should serve as one of reference for the elementary student, and to this end it should be provided with an efficient index.

Under the second of the above titles there is in circulation a small volume of 238 pages, which is for the most part a translation of the third German edition of some notes printed for special use in Prof. Selenka's classes. They were originally intended "to serve for the reception of sketches and notes during lectures and practical work, and at the same time to facilitate a review of classification"; as reproduced in English, they are extensively interleaved, the blank paper being provided "to receive brief synopses from voluminous lecture notes" (*sic!*), "or, in some cases, definitions of families and smaller subdivisions." Numerous "additions and revisions" have been made by the translator, chief among them being a concise and well-compiled appendix of five to six pages, with a table, dealing with certain principles of distribution. This is, in some respects, the most satisfactory portion of the work, although in itself a literary essay such as might be produced by any intelligent student who had mastered the broad principles of his subject and could command scissors and paste. On turning to the body of the volume, we read, among other things, the following:—*Fierasfer* is parasitic upon Holothuria, *Exocoetus* is a Malacopteroid, *Diprotodon* was a springing Marsupial, *Glyptodon* was insectivorous. The *Rodentia* are diagnosed as deciduate Placentalia, with "$\frac{1}{1}$ chisel-shaped continuously growing incisors, and three to six back teeth with transverse folds of enamel"; while of the Apes it is said that the alisphenoids are "united with the parietals" (*Platyrrhini*) or are not united with them (*Catarrhini*), no mention being made of the malar. A novel version this of the discovery of Joseph and Forbes.

Upon the strength of his "additions and revisions" the translator practically claims a joint authorship. It will, we believe, be generally admitted that of all translations of the kind before us there are none which excel those into French. We have no wish to force comparisons, but we cannot refrain from contrasting the translator's work and assumed position towards Prof. Selenka's little volume, with, say, that of Carl Vogt towards Gegenbaur's "Grundzüge" or of Moquin-Tandon towards Claus's "Handbuch," the two finest translations of zoological treatises the world has yet seen. So efficiently has the translator of the last-named supplemented and extended the original that the French edition has become a new work; but, this notwithstanding, the translator is content to be regarded as such, and such alone. The endeavour, on the part of a translator or of an adapter, to pose as joint author of a work in connection with which he has performed a mere clerical labour is no new device; but it is one which cannot be too strongly denounced. It betokens, to say the least, an unfairness on the part of the junior which, it would not be difficult to show, has, in the past, amounted to an imposition upon the generosity of the senior, and to the infliction of a pang whose effects have been ineradicable. The translator of the work before us would have done well if, instead of having endeavoured to extend its scope, he had verified the accuracy of the statements which it contains. Why he should not have corrected errors such as those we have cited we are at a loss to understand; and he certainly should have avoided the addition of fresh defects. These seriously mar the pages of a book which, if revised anew, might be useful to the student.

G. B. H.

OUR BOOK SHELF.

The Hand-book of Folk-Lore. By G. L. Gomme. (London: Published for the Folk-Lore Society by D. Nutt, 1890.)

THE energetic Folk-Lore Society has just issued a handbook for the guidance of collectors and workers on folk-

lore. The preparation of the work was intrusted to Mr. G. L. Gomme, the Director of the Society, who, it will be remembered, has recently published a very suggestive book entitled, "The Village Community," and is the author of numerous papers on folk-lore. Mr. Gomme has been assisted in the preparation of the book by several members of the Folk-Lore Society. At the outset Mr. Gomme defines "What folk-lore is," and after some judicious remarks he states: "From what has been advanced it may be conceded that the definition of the science of folk-lore, as the Society will in future study it, may be taken as follows, '*the comparison and identification of the survivals of archaic beliefs, customs, and traditions in modern ages*.'" The subject is divided into four sections, each of which is further subdivided. The principal divisions are as follows:—(1) Superstitious belief and practice: including superstitions connected with natural objects, goblindom, witchcraft, leechcraft, magic, beliefs relating to future life, &c. (2) Traditional customs: festival customs, ceremonial customs, games, local customs. (3) Traditional narratives: nursery tales, *Märchen*, fables, creation, deluge, &c., myths, ballads and songs, place legends. (4) Folk sayings: nursery rhymes, riddles, proverbs, nicknames, place rhymes. For each division there is a short account of the scope of that especial branch of folk-lore treated in a comparative manner, illustrations being drawn from the most varied sources; by this means the reader is enabled to gain a comprehensive view of the subject; then follow a number of questions for the guidance of collectors. The plan of the book is much the same as that of the well-known "Anthropological Notes and Queries," and it does for those interested in folk-lore what that estimable little work has done for the traveller—it instructs him what to do and how to do it. The author appeals not only to those who have the opportunity or the inclination to mix with "folk" (or "the less advanced classes in cultured nations"), but also to those who prefer to or possibly only can work in the library, and valuable hints are given to the latter, and two specimen tabulation forms are inserted. It is to be hoped that the publication of this carefully prepared hand-book will give definiteness of aim to the numerous people who take an interest in all "relics of an unrecorded past," and who would gladly collect information if they knew what was worth recording, how to arrange their facts, and where to send them to. Such information this book supplies, and the cost, being only half-a-crown, places it within reach of all.

Physikalische Krystallographie. By Dr. M. Liebisch, o.ö. Professor der Mineralogie an der Universität Göttingen. (Leipzig: Veit and Co., 1891.)

THIS book furnishes another example of the profound learning and patient industry of the German man cf science. Dr. Liebisch, who is well known as one of the editors of the *Neues Jahrbuch für Mineralogie, Geologie, und Palaeontologie*, has recorded in the large octavo volume (of 614 pages) lying before us, a mass of investigations and abstruse mathematical proofs and deductions dealing with the physical properties of crystals. The subject is treated throughout in such a way as to bring out clearly the interesting relations existing between the physical phenomena and the geometrical symmetry of crystalline substances.

With this end in view, the author treats consecutively of homogeneous deformations, of the nature and orientation of isothermal planes, of thermo-electricity, of magnetic induction, of dielectrical polarization, and of pyro- and piëzo-electricity in crystals. The last part of the work deals with the elastic properties of crystals and with the remarkable applications of the theory of the elasticity of crystals on the changes in double refraction produced by pressure, concluding with an account of the investigations on the elastic deformation of dielectrical crystals in the electrical field and the electro-optic phenomena observable in piëzo-electrical crystals.

Not the least valuable part of the book is the series of beautiful plates at the end of the volume, illustrating pyro-electrical phenomena and the phenomena of interference in doubly refractive crystals, the figures being prepared from photographs made by the author. Worthy of note is the fact that Dr. Liebisch has succeeded in photographing interference phenomena displayed in the monochromatic light of the sodium flame.

Valuable as the book is in its completeness and accuracy of information, there are few, we venture to think, who possess sufficient mental digestive power to assimilate its learned contents. Even the professed crystallographer will no doubt find within its pages some rather heavy reading.

Anleitung zur Darstellung chemischer Präparate: ein Leitfaden für den praktischen Unterricht in der anorganischen Chemie. By Dr. Hugo Erdmann. (Frankfurt-a-M.: Verlag von H. Bechhold, 1891.)

A CONSIDERABLE evil of the present system of education is that the variety of subjects which a student has to pursue is increased, while the time at his disposal is, in many cases, less than it used to be. And as the examination is the only criterion of his success, both students and teachers are anxious to exclude all work that has not an immediate bearing upon this final test. In the study of chemistry, this state of affairs is leading to the exclusion of some of the most important branches of the subject, because practical examinations are invariably of an analytical character. When a student takes up organic chemistry, he is generally set to make a few preparations, and so accustom himself to work with larger quantities than a gram or two. But, although such practice is just as valuable and necessary to the student of inorganic chemistry, he very rarely enjoys a similar privilege. Dr. Erdmann has endeavoured to supply this deficiency so far as a guide-book is concerned, and though in many cases his directions are rather meagre, he has put together an excellent series of instructions which refer to compounds of all the commoner elements. The word inorganic is not too literally interpreted, for we find that oxalic acid crystallized and dry, ethyl bromide, hydrocyanic acid, urea, thiophen, Prussian blue, and some acetates are included. The volume of 71 pages concludes with a few useful notes concerning apparatus and processes, and gives directions for obtaining streams of several of the commoner gases without the aid of heat.

Analysis of a Simple Salt. By William Briggs, F.C.S., and R. W. Stewart, B.Sc. Lond. (London: B. W. Clive and Co.)

IN compiling a guide, it is a good thing to go to the best sources for information, but it is a graceful thing to acknowledge whence the information has been gathered. We do not for a moment suggest that every paragraph should be encumbered with a statement of the various authorities that support the facts expressed in it; but when the manner and order of treatment, and very often the actual verbiage, are taken from a volume that is still in current use, it is certainly due to all who have an interest in that volume, that the compilers shall acknowledge their indebtedness. If the compilers had followed more closely still the scheme for the "preliminary examination in the dry way" as detailed in Valentin's "Qualitative Analysis," we think they would have improved their method. And it may also be remarked that, to prepare always neutral solutions in searching for acids, leads to much waste of time, and especially so when the substance for analysis is a simple salt. A few specimen analyses are added at the end of the book. These, even when judged of by the tables given in the body of the work, are not

always complete, and sometimes inaccurate. Overlooking the points already referred to, the volume is likely to prove a useful and trustworthy assistance to those for whom it is especially intended.

Magnetism and Electricity. By J. Spencer, B.Sc. (Lond.), F.C.S. (London: Percival and Co., 1890.)

THIS work will form a useful class-book when the elementary parts of these subjects are being taught. It is divided into three parts, the first dealing with magnetism, while the other two treat of electricity, subdivided into frictional and voltaic electricity. The idea throughout has been to arrange a series of easy experiments, which the teacher or student can with little difficulty perform. In each case good instructions are given, and, following these, are the conclusions which can be derived from them. The scope of the subjects under treatment is limited to the elementary stage of the Science and Art Department, and at the conclusion of each chapter are arranged exercises, among which are some that have been previously set by that Department. In the appendix will be found the syllabus, and questions that have formed the subject of examinations of the Department during the last two years, which should be found useful to both teachers and students.

The Elements of Euclid. Books I. and II. By Horace Deighton, M.A. New Edition, Revised. (Cambridge: Deighton, Bell, and Co., 1891.)

THE chief alterations made in this new edition consist in the introduction of symbols and abbreviations; and in order that beginners should not be confused with them at the commencement, the first fifteen propositions are proved without their use, with the exception of the symbols for therefore, because, and equal to. For several of the exercises the author has substituted others which he considers more suitable, and, in addition, some new ones have been inserted. Of the two books dealt with, Book I. is treated very fully, and each proposition is followed by a considerable number of easy examples, which, if worked out by the student, should give him a thorough knowledge of the propositions preceding them. A most useful collection of propositions is added, with which the reader is recommended to make himself familiar.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Mr. Wallace on Physiological Selection.

I WILL endeavour, as briefly as possible, to justify what I have said elsewhere touching "the peculiar position to which Mr. Wallace has eventually gravitated with reference to my views." For a fuller statement I must refer to the *Monist*, vol. i. pp. 1–20.

It is quite true, as he says, that "in his original paper, and in the summary of it published in NATURE, Dr. Romanes adduced variations in regard to fertility and sterility as the fundamental fact in physiological selection." And this is exactly what Mr. Wallace himself has done in his "alternative theory." Taking it for granted that these variations must always occur—as in my "original paper" I said they probably "most frequently" occur—in a whole race or strain, his theory seeks to explain, (1) the *causes* of such variations, and (2) their *effects* in furnishing an important condition to the origination of specific types, and this exactly in the manner that the theory of physiological selection had previously exhibited. Space forbids any long quotations, and therefore on the present occasion I will confine myself to transcribing two sentences from the paragraph in which he very correctly summarizes his theory, viz. the sentences which deal with the *effects* of these "variations in regard to fertility and sterility."

"The preceding argument, it will be seen, depends entirely upon the assumption that some amount of infertility characterizes the distinct varieties which are in process of differentiation into species. . . . It is by no means necessary that all varieties should exhibit incipient infertility, but only some varieties; for we know that, of the innumerable varieties that occur, but few become developed into distinct species; and it may be that the absence of infertility, to obviate the effects of intercrossing, is one of the usual causes of their failure."

Here we have the whole essence of "physiological selection" in a nutshell. The first sentence conveys the "fundamental fact;" the second indicates its possibly important consequence in permitting the origination of species by preventing the effects of intercrossing. Why, then, does Mr. Wallace not perceive that this is the whole essence of physiological selection? As far as I can understand, the reason appears to be that he deems his variations in the direction of cross-infertility to differ from mine, in that while his may be associated with other conditions or causes of modification, mine, as he now repeats in NATURE, are supposed always to act "alone in an otherwise undifferentiated species." Now, not only did I expressly guard against this interpretation in my "original paper," but in all my answers to Mr. Wallace's criticisms which have since appeared I have over and over again corrected the mis-statement on his part; and I am the more surprised that he should again reproduce it as the sole basis of his present reply, inasmuch as some of the passages which he quotes for this purpose from the paper in question themselves furnish the needful correction in their own immediate context.[1] Moreover, not only have I thus from the first fully recognized the sundry other causes of specific change with which the physiological variations may be associated; but Mr. Gulick has gone into this side of our common theory much more fully, and elaborately calculated out the high ratio in which the differentiating agency of any of these other causes must be increased when assisted by—*i.e.* associated with—even a moderate degree of the selective fertility, and *vice versâ*. Therefore, it is simply impossible for Mr. Wallace to show that "our theory" differs from his in this respect. Yet it is the only respect in which his reply alleges any difference.

I do indeed myself believe that in many cases the physiological variation may arise alone, in an otherwise undifferentiated species; but from the first I have always maintained that it makes no essential difference to the theory in what proportional number of cases it has done so. And in a forthcoming treatise I shall be able very completely to dispose of Mr. Wallace's "two carefully considered cases," whereby he claims to have proved that the possibility of physiological selection ever working alone is "absolutely unfounded."[2] At present, however, the point is that, even if I am wrong in supposing that physiological selection can ever act alone, the principle of physiological selection, as I have stated it, is not thereby affected. And this principle is, as Mr. Wallace has re-stated it, "that some amount of infertility characterizes the distinct varieties which are in process of differentiation into species"—infertility, whose absence, "to obviate the effects of intercrossing, may be one of the

[1] For instance, to take only the first of his "few quotations," he reproduces by itself the following sentence:—"It becomes almost impossible to doubt that the primary specific distinction [meaning sterility] is, as a general rule, the primordial distinction." Now this, be it remembered, is quoted for the expressed purpose of "making it absolutely clear that Dr. Romanes's theory of physiological selection, so far as it had any originality, *was founded on the supposition of sterility alone, arising in an otherwise undifferentiated species*." Yet, if Mr. Wallace had but read the context, he would have seen that this statement is directly contradicted. For the very next sentence is as follows:—"I say as a general rule, because the next point which I wish to present is, that it constitutes no part of my argument to deny that in some—possibly in many—cases *the primary distinction may have been superinduced by the secondary distinctions*." By the "primary distinction" Mr. Wallace correctly understands me to mean cross-infertility between allied species, while by "secondary distinctions" it is explained in the same place that I mean specific characters of all other kinds. The passage then goes on, through a number of pages, to show "how natural selection, or any other cause [which is concerned in the differentiation of species], may have induced this particular kind of variation in the reproductive system by its operations on other parts of the organism," and the ultimate conclusion of this lengthy argument is: "Thus, we see, it really makes no essential difference to my theory whether it be supposed, in any given case, that the primary distinction was prior, or subsequent to the secondary distinctions." Comment appears needless.

[2] These cases consist in some exceedingly simple arithmetical computations, which, as I shall hereafter show, rest upon erroneous data. Mr. Fletcher Moulton, who has kindly gone into the matter in a really efficient manner from the mathematical point of view, reports, to use his own words, "an enormous difference from Mr. Wallace's results."

usual causes of their failure to become developed into distinct species."

This, I repeat, is the essence of physiological selection; and any "originality" which my views upon the subject present consists in recognizing the "fundamental fact" set forth in the first of the two sentences above quoted, together with its consequence as set forth in the second. Before Mr. Catchpool published the theory in these columns, no one—with the partial exception of Mr. Belt—had perceived this factor of organic evolution; and while, for about the sixth time, repudiating the grotesque "originality" which Mr. Wallace continues to ascribe, I may conclude by observing that—personalities apart—it is to me a matter of satisfaction that he has now himself begun to perceive the existence and the importance of the factor in question.[1] GEORGE J. ROMANES.

Oxford, December 1.

The Tornado.

IN NATURE, vol. xlii. p. 612, there appears a notice of my book on "The Tornado," from "H. F. B." I must thank so high an authority for noticing the book; all I ask for is a full, free, and fair discussion of the facts presented. May I call attention to one or two points which may not be clearly understood?

(1) My object in writing the book was to bring together all the facts known regarding tornadoes, and to give a brief *résumé* of theories, as far as possible, showing the gradual development during the past fifty years.

(2) I have nowhere touched one of Prof. Ferrel's mathematical discussions of the problem. In some cases I have tried to show that there may be an interpretation of certain physical phenomena not exactly in accord with his own views.

(3) I have not denied a single thermodynamic principle. The quotations given by "H. F. B." are quite plain when the book is read as a whole. These do not refer to thermodynamic principles at all, but rather to the experiments made by Prof. Espy nearly fifty years ago, and whose results I have tried myself to check. I am sure that anyone who carefully peruses the book will be satisfied that there have been read into it statements or inferences which are not there.

(4) Above all, I have not advanced any visionary electric speculations regarding the generation of tornadoes. Rather I have tried to avoid that very thing. In the very quotation made by "H. F. B.," from p. 76, occur these words:—"It has been my purpose for many years to avoid, as much as possible, all speculations in considering air motions and the causes of atmospheric phenomena. This is especially pertinent when we consider electric action in the atmosphere. It is very difficult to believe that electricity has nothing to do with our thunderstorms."

It is a significant fact that "H. F. B." begins his quotation with this last clause, and from it tries to show that I have adopted an electric hypothesis. I cannot quite see why he did not begin where he should have done. I have not given my name to any hypothesis at all, but have advanced a few facts which I hope may ultimately help us to build up the true view of tornadic phenomena.

The familiar lecture experiment of forming a cloud in a receiver by a stroke of the air-pump is given by "H. F. B." as an illustration of dynamic cooling. It would be quite interesting if some one would compute the amount of work done by the air in this case, premising that the stroke of the pump is made quickly enough to form a vacuum into which the air from the receiver rushes. Tyndall says: "Mere rarefaction is not of itself sufficient to produce a lowering of the mean temperature of a mass of air." It is quite well known that the work done in this experiment is not that of driving out a piston, but is rather the very slight work needed to impart a motion to the molecules in the receiver—in other words, to drive out these particles from the receiver.

As to whether a dense and cold stratum of air in motion can overrun a warmer stratum, I have to say that this question has been negatively settled in this country. While such a condition might be possible in quiescent air, and has been observed in balloon voyages, yet in these cases there was no disturbance of the atmospheric equilibrium. In balloon voyages I have myself found a distribution of temperature in a vertical direction, which, according to theory, should have given rise to a violent tornado, but there was no marked disturbance. I have faith to believe that in the near future there is to be a marshalling of facts which shall establish true views of storm-generation; and even now there are many intelligent men who have grown restive under the present pure theories and mathematical analyses of atmospheric phenomena. To my mind, the Dr. Hann agitation has done a great deal to open the eyes of orthodox meteorologists, and even "H. F. B." seems to be in a little doubt as to the final outcome of his views in relation to orthodox meteorology. It seems to me all persons who are studying storm-generation and movements are realizing the absolute need of a solid groundwork of fact on which to base our views, and this is the great point that I have been contending for these many years.

Washington, D.C., November 18. H. A. HAZEN.

PROF. HAZEN can alone speak as to what views he intended to advocate; a reviewer can only take count of such as are expressed or implied in the work he reviews, and the present writer is unable to see in the above letter any evidence that the quotations given from Prof. Hazen's work were not fairly representative of his text, or that they fail to justify the comments upon them. That he is not alone in his inference that Prof. Hazen "appears to regard as inapplicable to the movements of the atmosphere, those laws of thermodynamics that are based on the results of Joule's labours," is shown in the following extract from Prof. J. Hann's paper in the September number of the *Meteorologische Zeitschrift*:—"Da Herr Hazen so ziemlich alle Grundlagen, auf welchen man die Meteorologie in neuerer Zeit mit Sicherheit weiter ausbauen zu können vermeint, leugnet oder in Zweifel zieht, darunter selbst allgemein anerkannte physikalische Gesetze, wie. z. B. die adiabatische Temperatur-Aenderungen in feuchter Luft, so scheint es zunächst allerdings überflüssig, sich mit ihm in eine Kontroverse einzulassen, da der Boden für eine Verständigung gänzlich fehlt." The passage referred to in this remark is one of those quoted in the present writer's review.

H. F. B.

Araucaria Cones.

I AM drawn to add, with your permission, a few words to what has already been written on the subject of Araucaria cones, by noticing that all your correspondents speak of trees situated in the south of the British Isles, or, at least, not further north than Cambridge, whereas it may be of equal, if not greater, interest to the Duke of Argyll and others to know something of the behaviour of the Araucaria in the north of Scotland. It also seems to me unfortunate that many of the correspondents have omitted to mention the most interesting point concerning the fruiting of the Araucaria, viz. the monœcious or diœcious character of the trees they describe. Loudon, in his well-known book on the "Trees and Shrubs of Great Britain," pronounces the Araucaria to be diœcious. At that time knowledge could only be gained of it on its native hills. In the "Manual of Coniferæ," published by J. Veitch and Sons, is figured a branch from the monœcious tree at Bicton with both pollen and ovule-bearing catkins. It would be interesting to learn if many of the

[1] It is, perhaps, desirable to add, as already stated elsewhere, that I entertain no doubt at all touching the unconscious or unintentional nature of the "adoption." Nevertheless, I may further add, the adoption itself is so manifest, that several eminent men of science wrote to me on the subject when first his work on "Darwinism" appeared. Among the mildest of their comments are:—"Mr. Wallace has treated you very badly. After having set up a caricature of your theory, he adopts the theory itself, pure and simple." But of more importance is Mr. Gulick's opinion, seeing that he was the first to conceive, though the last to publish, the theory of physiological selection. As soon as he had read "Darwinism," he wrote me from Japan a long letter, the substance of which may be gathered from the first two sentences, as follows:—"Mr. Wallace has most certainly adopted the fundamental principles of our theory. He takes our principles, which in the previous chapter he has combated; but he makes such disjointed use of them that I am not willing to recognize his statement as an intelligible exposition of our theory." More recently he sent to the *American Journal of Science* a paper, which he summarizes thus:—"Mr. Wallace's criticism of the theory of physiological selection is unsatisfactory: (1) because he accepts the fundamental principles of that theory on pp. 173–79, in that he maintains that without the cross-infertility the incipient species there considered would be swamped; (2) because he assumes that physiological selection pertains simply to the infertility of first crosses, and has nothing to do with the infertility of mongrels or hybrids; (3) because he assumes that infertility between first crosses is of rare occurrence between species of the same genus, ignoring the fact that, in many species of plants, the pollen of the species is prepotent on the stigma of the same species when it has to compete with the pollen of other species of the same genus; (4) because he not only ignores Mr. Romanes' statement that cross-infertility often affects 'a whole race or strain,' but gratuitously assumes that the theory of physiological selection excludes this 'racial incompatibility' (which Mr. Romanes maintains is the 'more probable form'), and bases his computation on the assumption that the cross-infertility cannot be associated with any other form of segregation.

trees now under cultivation are monœcious, the tree perhaps adapting itself to the enforced solitude of its new abode in gardens. However, it appears at present to be both monœcious and diœcious, like several other Coniferæ.

In the grounds at Beaufort Castle, near Inverness, are several specimens of this Araucaria, producing only male or pollen-bearing catkins. One, a particularly fine tree, has borne such pendent cones for several years past. Last week I had the opportunity of inspecting a rather famous Araucaria at Conan, the seat of Sir Kenneth Mackenzie. This is said to be one of the first three specimens introduced into Scotland; the other two at Edinburgh were, I believe, killed by the severe winter of 1860. The tree at Conan has lost all its lower branches, and has grown but little of late years; it, however, yearly produces the erect ovule-bearing cones, and about twelve years ago, these contained fertile seed. Specimens grown from the seed of that year are still thriving at Gairloch, on the north-west coast of Scotland. No fertile seeds have been produced since; and as there was no other specimen certainly within several miles that could have produced the necessary pollen, we must conclude in the absence of direct evidence that this is a diœcious tree, and produced the more insignificant male catkins on the one occasion only. I therefore need hardly point out that unless the Araucaria at Inveraray is also provided with male flowers, or some other specimen similarly provided grows in the neighbourhood, the Duke of Argyll may assuredly expect his lawn to be strewn next year with only empty seed-vessels.

In answer to Mr. Gardiner, I may remark that I could detect no difference in habit or foliage between the diœcious male tree at Beaufort and the monœcious one at Conan; the latter, however, is so much damaged as to render comparison difficult.

ADRIAN WELD-BLUNDELL.

The Abbey, Fort Augustus, November 30.

YOUR correspondent, Mr. A. D. Webster, in your issue of November 20 (p. 57), states that the male catkins of the above tree are extremely rare as compared with the fruiting cones. If this is the case, though my own observations would have led me to the contrary opinion—the following instances may be of interest. I have observed the male catkins on a tree in the cemetery of this town for two or three successive years, in considerable quantities. In the grounds at The Elms, Houghton, Hunts., there is a tree which for several years past has borne large quantities of the male "amenta," giving to it, as your correspondent describes, a very striking appearance. Another tree in the same grounds has this year produced a single specimen of the same nature, while a third, of the opposite sex, is also developing a fruiting cone, which will doubtless, in the near proximity of the pollen-bearing ones, perfect its seeds.

Among specimens of the male catkins that I possess from the latter place is one which is "double," the floral axis being bifid. It has occurred to me that this may be some slight indication as to the much-vexed question of the morphology of the "amenta," whether each consists of a series of *monandrous* flowers or constitutes a single *polyandrous* one; the above monstrosity seeming to point towards the former, as the bifurcation of the axis of an *inflorescence* is a common phenomenon, that of a single *flower* being, on the contrary, much rarer.

I am not sure whether I should be right in generalizing from the comparatively few fruiting examples I have seen, but in the cases which have come under my observation the female trees have been more distantly branched than the male, where the ramification is considerably closer and more luxuriant.

Northampton, December 3. H. N. DIXON.

Dry-rot Fungus.

THE "beautiful growth of fungus covering the wall and floor (in a wine-cellar) to a depth of 4 inches, suggesting cotton-wool in form and colour," referred to by "M. H. M.," is the destructive dry-rot (*Merulius lacrymans*), and I would advise your correspondent to make war upon it without delay. The cotton-wool form is an early stage of the fungus. If neglected, it will in a few months develop a leathery sheet, sending out tough leathery cords a quarter of an inch thick, with spore-bearing folds of a rusty colour. These spores will scatter themselves all over the cellar, and will be difficult to eradicate. The mycelium of the fungus buries itself in any kind of wood, especially deal, runs rapidly down the longitudinal fibres, and, as it goes, destroys the "nature" of the wood, so that it snaps and crumbles under the slightest pressure. I have had to deal with this pest in a range of cellars with a timber roof, and have found the best remedy to be repeated applications of corrosive sublimate dissolved in methylated spirit freely painted on the timber, walls, or floor, wherever the "cotton-wool" makes its appearance. I had to cut away 8 feet in length of a 10-inch Memel beam which was permeated by the mycelium, and rotten to the core. Between the end of this beam and the back of the recess in the brick wall in which it rested was a vacant space filled with the mature fungus full of spores. This was two years ago. I have been fighting the fungus ever since with the corrosive sublimate, and have nearly exterminated it. The first appearance of the cotton-wool should be attacked without delay. F. T. MOTT.

Birstal Hill, Leicester.

The Effect of Fog on Plants.

As my name appears somewhat prominently in your note on the important inquiry into the effect of fog on plants, may I explain that the experimental investigation of the subject from a botanical point of view is entirely in the able hands of my friend, Dr. Oliver?

I am prepared, as stated in the Scientific Committee's circular, to examine any specimens of plants affected by fog which may be sent to me, but my share in the work does not go beyond this.

The inquiry is of very great interest, both to horticulturalists and botanists, and I am glad that it has been noticed in the columns of NATURE. D. H. SCOTT.

Royal College of Science, South Kensington, London, S.W., December 6.

Great Waterfalls.

WOULD you allow me to supplement my inquiries published in last week's NATURE (p. 105) by asking for a description of the Pambam-arivy Falls in India, of which I have only the following brief note:—"In the Travancore Hills between Tinevelly and Travancore is situate the magnificent Pambam-arivy, or Snake Fall. It is a double fall, descending in the first plunge from the cliff edge 1200 feet, and it can be seen from a distance of forty miles." ARTHUR G. GUILLEMARD.

Eltham, Kent, December 9.

A Band of Light.

THE account of the so-called comet that was seen by Mr. Eddie at Grahamstown (see NATURE, November 27, p. 89) reminds me of the phenomenon seen some years ago in this country during an auroral display. A band of light, in shape somewhat resembling a comet, was seen to move across the sky, rising in the north-east and disappearing in the north-west; it moved, however, much faster than the comet-like body lately observed, being in sight, as far as I remember, only one or two minutes. C. C.

Trinity College, Cambridge.

Some Habits of the Spider.

THE following record of the habit of certain spiders, alluded to by "A. S. E.," is the only one known to me. "He saw great spiders with crowns and crosses marked on their backs, who sat in the middle of their webs, and when they saw you coming, shook them so fast that they became invisible" (Kingsley, "Water Babies," p. 40.) W. E. H.

BOTANICAL ENTERPRISE IN THE WEST INDIES.

DURING the last twelve years considerable effort has been made to enlarge the sphere of action of the botanical organizations in the West Indies. At the beginning of the period there were only two botanical establishments in this part of the world, one at Jamaica and the other at Trinidad. Since that time an important botanical garden has been successfully

established at British Guiana, and lately a scheme of botanical stations has been put forth to suit the circumstances of the smaller islands, described in these columns (vol. xxxv. pp. 248–250). This scheme of botanical stations for the West Indies has been very carefully and assiduously fostered at Kew, and it appears now as if it were likely to be fully carried out. It was felt that the smaller islands under present circumstances could not support any considerable organization of their own, but, on the other hand, if they joined together and affiliated their stations to one or other of the larger gardens there were good grounds for believing that satisfactory results would be attained.

Stations have been already established at Grenada, St. Vincent, St Lucia, Barbados, and Antigua, while others are in course of being established at Dominica, and St. Kitts, and Nevis. The curators of these stations are carefully trained men (mostly from Kew), whose chief qualification is a thorough knowledge of horticultural methods as applied to tropical plants. They devote themselves to the maintenance of the stations as centres for the growth and distribution of economic plants, and they carry on experiments with the view of improving old or introducing new industries. It may readily be gathered that the objects of these stations are thus of a very simple and unassuming character. They necessarily work within narrow limits. They cannot give much attention to plants of a purely decorative character, or, again, to the maintenance of large areas under cultivation as pleasure grounds. The main object in view is to meet the special circumstances of the West Indies at the present time, and do all that is possible to encourage a diversified system of cultural industries so that they may not suffer so much as hitherto from fluctuations of prices in the chief staples.

It is believed that the Botanical station scheme will eventually meet the wants of each island in a simple and economical manner. It will become the basis of a federation for purely economic purposes likely to be generally beneficial both to the European planter and the Negro small proprietor. The stations will, it is hoped, have the occasional supervision of the heads of the Botanical Departments at Jamaica and Trinidad, and they will draw from thence such supplies of seeds and plants as may be required from time to time for their special wants.

In the organization of the scheme successive Secretaries of State for the Colonies have taken a warm interest, while the elaboration of the details has necessarily to a large extent fallen upon Kew. As the scheme took root, the discussion of these details involved a rather heavy burden of correspondence. It was eventually felt to be advisable both by the West Indian Governments immediately concerned and by the Colonial Office, that one of the Kew Staff should proceed to the West Indies in order to advise on the spot as to the various questions incidental to the successful start of the various stations. The mission was accordingly intrusted to the Assistant-Director, Mr. Morris, who has had a considerable experience of planting industries both in the Old World and in the New. Mr. Morris left by the mail of November 12, and will be absent about three months.

NOTES.

MR. W. F. R. WELDON, F.R.S., has been appointed by the Council of University College, London, to the Jodrell Professorship of Comparative Anatomy and Zoology, which was held for sixteen years by Prof. Ray Lankester. Mr. Weldon is a Fellow of St. John's College, Cambridge, and is a lecturer on Invertebrate Morphology to the University of Cambridge.

THE Council of the Royal Geographical Society has agreed to grant £200 to Mr. Theodore Bent to aid him in exploring the now famous ruins in Mashonaland.

ACCORDING to the *Colonies and India*, the important post of Superintendent-General of Education in the Cape Colony will fall vacant at the end of the present year, owing to the retirement, after 30 years' service, of Sir Langham Dale, K.C.M.G. This gentleman was in 1848 selected by Sir John Herschel to be Professor of Classics in the South African College, Cape Town, and in 1859 was appointed to the office which he now holds. He also became Vice-Chancellor of the University of the Cape of Good Hope in 1873. The value of the appointment is £1000 per annum.

THE Whitworth Trustees have added to their gifts to Manchester 12½ acres of land, known as the Stanley Grove Estate, purchased by them for £27,000, for a hospital under the direction of the Owens College. The site is alongside the Whitworth Park.

ACCORDING to the will of the late Sir Edwin Chadwick, a trust fund is to be applied as the trustees shall direct for the advancement of sanitary science. By a codicil to his will, the testator suggests that the trustees may offer a cup of the value of £20 to be given annually, for a term of years, to sanitary authorities at home or abroad showing the greatest reduction in the death-rate of the population in their district. He suggests also the offer of a silver medal to the school teacher showing the best mental results of the half-time principle, and a gold medal for district school managers obtaining the best results in subjects named.

LAST week we referred, in a leading article, to a Conference convened by the National Association for the Promotion of Technical and Secondary Education for the purpose of considering the best means of utilizing the new fund placed at the disposal of County Councils under the Local Taxation Act of this year for the promotion of education. The Conference met on Friday, December 5, and was largely attended. Lord Hartington, who occupied the chair, opened the proceedings with a short speech, in which he urged that the best way of securing the fund will be to see that it is used for the purpose for which it was originally granted, by stimulating existing institutions in the work they are now doing, by adding a scientific and practical side to schools, and providing new schools where such do not now exist. There are, he pointed out, many ways in which the grant can be applied, and it is clear that one cut-and-dried system cannot be applied throughout the country. The system in urban districts must be different from that established in agricultural districts. The secretaries presented a report on the working of the Technical Instruction Act and the Local Taxation Act; and afterwards there was a most careful discussion of all aspects of the question which the Conference had met to consider. No formal motions were adopted, as several members of the Conference felt that they could not commit their County Councils to definite resolutions. In the course of the discussion Sir W. Hart Dyke assured the Association of his willingness in every possible way to co-operate with them. Mr. Mundella said the best result of the meeting would be if it insured the continuance of the grant in the channel in which they wished it to go. If this money was not promptly utilized for education, it might revert to the Treasury. No doubt embarrassment might be felt in some localities, but he would recommend in such case that the money should be placed in a suspense account, which the Act enabled them to do. The total absence of provision for intermediate education had been a grave defect in our system. Wales had appropriated the whole fund, and stood higher in respect of intermediate education than any other part of the country.

THE Second International Ornithological Congress will be held at Buda-Pesth in May 1891. The Hungarian Committee nvite all specialists and members of Ornithological Societies to

attend the meeting. There will be excursions to various parts of Hungary of ornithological interest. An official programme will shortly be published.

AT a meeting of the State Commissioners on Niagara Falls, held at New York on December 8, a report was presented from the State Engineer upon the survey which has just been made. According to a telegram sent through Dalziel's Agency, this report gives particulars of the recession of the Falls since 1742, when the first survey was made. It shows that the total mean recession of the Horse-Shoe Falls since 1742 has been 104 feet 6 inches. The maximum recession at one point is 270 feet. The mean recession of the American Falls is 30 feet 6 inches. The length of the crest has increased from 2260 to 3010 feet by the washing away of the embankment. The total area of recession of the American Falls is 32,900 square feet, and that of the Horse-Shoe Falls 275,400 feet.

DR. BERGHAUS, the well-known geographer, died at Gotha on December 3.

THE death is announced at Warsaw of Herr Anton Waga, one of the most eminent Polish naturalists, at the age of 91.

MR. R. D. OLDHAM, of the Geological Survey of India, has been attached to the military expedition into the Zhob country. His principal duty will be to inquire into the reported oil-fields in the Shirani country. The last specimen examined by an expert has been declared of most excellent quality.

A DESPATCH from Mexico states that an earthquake, lasting some minutes, occurred there on the evening of December 2. The shock was the severest felt for years, and the inhabitants rushed into the streets in terror.

EXTENSIVE subsidences of cliff have taken place at Walton-on-Naze, Essex. In one spot a surface area of 100 square feet slid bodily down to the beach, carrying with it part of a road leading to Frinton. Opposite the Marine Hotel, Walton, where the cliff is lower, a chasm large enough to berth a Thames steamboat has opened in the sea-front. The *Times* says that landslips have resulted from the loosening of the soil of the cliff through the action of springs, rather than from the direct attack of the sea.

THE Decimal Association has issued a pamphlet containing a popular explanation of decimal coinage, weights, and measures, by Sir Guilford Molesworth and Mr. J. Emerson Dawson. The information is condensed as much as possible under different headings, all examples and evidence in support of the statements put forward being relegated to appendices.

AT the meeting of the American National Academy of Sciences at Boston on November 11, 12, and 13, the following papers were presented:—On the primary cleavage products formed in the digestion of the albuminoid, gelatine, by R. H. Chittenden; on the classification and distribution of stellar spectra, by Edward C. Pickering; on the relation of atmospheric electricity, magnetic storms and weather elements, to a case of traumatic neuralgia, by R. Catlin; on the growth of children studied by Galton's method of percentile grades, by Henry P. Bowditch; on electrical oscillations in air, together with spectroscopic study of the motions of molecules in electrical discharges, by John Trowbridge; some considerations regarding Helmholtz's theory of dissonance, by Charles R. Cross; a critical study of a combined metre and yard upon a surface of gold, the metre having subdivisions to two millimetres, and the yard to tenths of inches, by W. A. Rogers; on evaporation as a disturbing element in the determination of temperatures, by W. A. Rogers; on the use of the phonograph in the study of the languages of the American Indians, by J. Walter Fewkes; on the probable loss in the enumeration of the coloured people of the United States, at the census of 1870, by Francis A. Walker; on the capture of periodic comets by Jupiter, by H. A. Newton; on the proteids of the oat-kernel, by Thomas B. Osborne; on the present aspect of the problems concerning Lexell's comet, by S. C. Chandler; the Great Falls coal-field, Montana, its geological age and relations, by J. S Newberry; notes on the separation of the oxides in cerite, samarskite, and gadolinite, by Wolcott Gibbs; on the relationships of the Cyclopteroidea, by Theo. Gill; on the origin of electro-magnetic waves, by Amos E. Dolbear.

A PAPER by Mr. W. B. Mason in the Transactions of the Seismological Society of Japan deserves the attention of all who take special interest in seismology. It contains a list of earthquakes recorded at telegraph stations in central and northern Japan from August 11, 1888, to December 31, 1889. Mr. Mason, while allowing for various sources of uncertainty in the observations, thinks that some results may be deduced from what are still meagre statistics. Thus of the 151 earthquakes recorded in Tokio only 89 were felt at the other telegraph stations. Some of those which were felt at all the stations seem to have been felt at almost exactly the same instant. In other words, there was no indication of a progression of the earthquake from point to point.

THE Brooklyn Institute is making most praiseworthy efforts to raise the standard of geographical teaching in the United States. Through its Department of Geography it will open, about the new year, an exhibition of specimens of the best geographical text-books, maps, atlases, globes, reliefs, models, telluria, and other apparatus used in the various countries of Europe and America in their courses of gergraphical instruction. The collection will be exhibited first in the building of the Brooklyn Institute, then in New York, Philadelphia, Boston, Baltimore, Washington, Chicago, St. Louis, and other great centres of population. The entire collection, with the exception of specimens which have been lent, will afterwards be arranged as a permanent exhibition in the building of the Brooklyn Institute. *Science*, from which we learn these details, says that, in connection with the exhibition, the Brooklyn Institute is collecting material for a comprehensive report which it will publish regarding the position and methods of geographical instruction in America and Europe.

THE Chief Signal Officer of the United States has just issued his Report for the year ending June 30, 1890. It shows that the meteorological department is yearly increasing in extent and importance; the duties include the issue of forecasts and storm warnings, the gauging of rivers for navigation and flood-warnings, the reporting of temperature and rainfall conditions for the cotton interests, the display of frost-warnings in the interest of agriculture, and the notification of advancing cold waves for the benefit of the general public. The average percentage of successful weather forecasts amounted to 82·6. Long time forecasts are also issued at the discretion of the forecast official, with successful percentages of 81·6 for 48 hours, and 80·5 for 72 hours. The demands for daily weather charts have increased to a remarkable extent; the subscription for them is two cents. a copy. The co-operation with the Meteorological Office in Paris is continued; each night a cablegram is sent to the latter office summarizing the synchronous meteorological observations, gales, derelicts and dangerous ice of the western Atlantic for the previous five days, together with the current weather conditions of the United States. Part of this information is regularly published in the Paris *Bulletin International*. The Report states that a card index of the stations in the United States at which meteorological observations have ever been taken is being prepared; when finished, it will afford a comprehensive history of the climatic observations in that country. Various important

scientific researches are being carried on, among them the accurate determination of the velocity of the wind, and more particularly its actual pressure during violent gusts, by Prof. Marvin; the relation of the dewpoint to the subsequent movement of the storm-centre, by Captain Allen; and the determination of the average destruction caused by tornadoes, by Prof. Hazen. The average number of persons killed by them is 102 yearly—which is considerably less than the number killed by lightning. These reports will be eventually printed as appendices to the General Report.

AT the meeting of the Linnean Society of New South Wales on October 29, Mr. J. J. Fletcher read a paper presenting contributions to a more exact knowledge of the geographical distribution of Australian Batrachia. While the broad facts relating to the geographical distribution of Australian Batrachia are fairly well known, much yet remains to be learnt respecting details, especially in regard to inland forms, since the species were originally described chiefly as they came to hand and without reference to the general batrachian fauna of the particular localities from which the types came, and with very few exceptions from coastal habitats. In Mr. Fletcher's paper, the first of what promises to be a useful series, three fairly complete collections are recorded from Dunoon, Richmond River (12 species); Guntawang, near Mudgee (13 species); and Dandaloo, Bogan River (10 species); and comparisons are instituted between the Batrachia of these localities and those of Port Jackson, the Blue Mountains, and Illawarra.

IN the report on the Chillingham cattle, read at the British Association in 1887, the following statement is made: "The cattle live on good terms with the red-deer, but they will not tolerate fallow-deer or sheep in the park." With regard to this statement, Mr. C. Oldham, of Ashton-on-Mersey, says in the December number of the *Zoologist* that on September 13 last, when at Chillingham, he watched a small party of fallow-deer for some time, as they fed on the hill-side with five of the white cattle; and the keeper, Michie, assured him that both red- and fallow-deer live in perfect harmony with them, and, if in any way alarmed or disturbed, generally seek safety in their company. The editor of the *Zoologist* adds that in Mr. Assheton Smith's park at Vaynol, near Bangor, where he has just spent some weeks, white cattle and red- and fallow-deer roam together, and no such hostility as that above referred to has ever been noticed.

THE collections of the Australian Museum, Sydney, are being steadily increased. In the report of the trustees for 1889, just received, it is stated that the principal purchases during the year were a collection of shells comprising some 15,000 species, and a collection of minerals, including specimens of gold from various parts of the world other than Australia. Several collecting expeditions were sent out with satisfactory results. The principal of these were:—(1) To Mount Ko-ciusko (Mr. R. Helms, collector), where an extensive collection of insects and other specimens from high altitudes, including many not previously represented in the Museum, was obtained. (2) To the Bellenden-Ker Ranges, north-eastern Queensland (Messrs. E. J. Cairn and R. Grant, collectors). This expedition obtained many rare mammals and birds of interest, including the remarkable tree kangaroo (*Dendrolagus lumholtzi*), and a new species of Petaurista, as well as a recently described new bower bird. (3) To Mount Sassafras, Shoalhaven District (Mr. R. Etheridge, palæontologist, and Mr. J. A. Thorpe, taxidermist, acting as collectors). This journey was undertaken with the view of obtaining some aboriginal remains, said to be concealed in a rocky recess at Mount Sassafras. The expedition was successful in obtaining the remains sought for, and in other ways. (4) To Blackhead, Illawarra District (Mr. A. J. North acting as collector). This was a short trip, to obtain certain fossils which were required to fill gaps in the collections. Abridgments of the reports of these various expeditions will be found in the first number of "The Records of the Australian Museum."

AT the request of Prof. Baird, Mr. William Palmer accompanied the United States Fish Commission schooner *Grampus* on her summer cruise in 1887 for the purpose of observing and collecting the fish-eating birds, together with their eggs and young. Mr. Palmer contributes to the Proceedings of the United States National Museum (vol. xiii., pp. 249–265) a record of his observations, and this has now been printed separately. He treats each species briefly. As all on board were interested in the matter, and frequently called his attention to birds seen by them, he believes his list contains all the species that came within a reasonable distance of the vessel. It might naturally be supposed that on a cruise of this character sea-birds would be found to be generally numerous, but such was not the case. With few exceptions, and these mainly on breeding islands, birds were very scarce, most of the many species having completed their migrations, and being in the far north or inland. As to the relative abundance of the species, Mr. Palmer places the most prominent in the following order: puffins, shearwaters, black hagdons, murres, and gannets. Good skins were made of the greater number of the species, and in many cases, also, eggs, embryos, and young in various degrees of plumage, were obtained. The localities visited were as follows: the Magdalen Islands and Bird Rocks, in the Gulf of St. Lawrence; St. John's; Funk Island; Seldom Come By; Cape Freels Penguin Islands; Toulinguet and Canada Bay, in Newfoundland; Black Bay and Mingan Islands; Southern Labrador, and Percé, Canada. The time covered was from July 8 to August 31.

MR. J. M. COODE records, in the new number of the Journal of the Bombay Natural History Society, the following instance of an exceptional method of hunting which the panther is occasionally forced to adopt. Mr. Coode was lately asked by the Patel of a village in the Amraoti district to accompany him one evening to a forest nursery of young bamboo shoots, to assist in killing a large boar which nightly visited the place and did immense damage. They waited for some time, when, just as is was getting dark, they heard the short guttural sound of a panther and heavy footfall of some running animal. The noises came nearer and nearer, until a nilghai and a panther could be distinctly seen against the sky-line, the former being chased by the latter. The nilghai kept moaning, and was evidently in an abject state of fear. The two ran round in a circle of about 160 yards diameter, within 30 yards of where the observers were standing, and passed them twice, both animals making their respective noises. They then disappeared, but Mr. Coode has reason to believe the nilghai got away.

THE following are the principal contents of the Journal of the Bombay Natural History Society (No. 3, vol. v.):—On new and little-known butterflies from the Indian region, with descriptions of three new genera of Hesperidæ, by Lionel de Nicéville (with plates); Bombay grasses (Part 2), by Dr. J. C. Lisboa; on new and little-known Hymenoptera from India, Burma, and Ceylon, by Major C. T. Bingham, Forest Departments, Burma (with plates); mules, by J. H. Steel; notes on the larvæ and pupæ of some of the butterflies of the Bombay Presidency, by J. Davidson and E. H. Aitken (with plates); the butterflies of the Central Provinces (Part 3), by J. A. Betham; notes on the economic botany of the Cucurbitaceæ of Western India, by Dr. W. Dymock; list of Chin-Lushai butterflies, by Lionel de Nicéville.

MESSRS. R. FRIEDLÄNDER AND SON, Berlin, have just issued a catalogue of books relating to Invertebrata. It includes a very

large number of important works on the special branch of zoology to which it relates.

In our fifth note, on November 27, p. 87, *for* Friana *read* Triana.

An important paper is contributed by MM. Barbier and Roux to the current number of the *Bulletin de la Société Chimique*, in which are described the final results of an elaborate experimental investigation concerning the relations between the optical dispersive power of the alcohols, ethers, and fatty acids, and their molecular weight and constitution. The relations brought to light, as might perhaps have been expected, are of the most regularly systematic character, and capable of expression in a few simple generalizations. The experimental portion of the work consisted in determinations of the refractive indices of the liquid in question at known temperatures, and for two widely separated lines of the spectrum of wave-lengths λ_a and λ_b. A measure of the dispersive power is then afforded by the amount of the difference between the values of the two indices thus obtained. It has also been shown that if the well-known expression of Cauchy, $n = A + \frac{B}{\lambda^2} + ...$, for the refractive index n be adopted, and for all practical purposes the expression appears to be sufficiently approximate, the constant B is a true measure of the dispersion. This value B is termed by MM. Barbier and Roux the dispersive power. In order, however, to eliminate differences due to the unavoidable differences of temperature at which the various determinations are made, the conventional expression $\frac{B}{d}$, where d represents the density of the liquid at the temperature of observation, is employed in the comparisons, and is called the specific dispersive power. The value of B is readily calculated from the two determinations of refractive index by means of the formula—

$$B = \frac{n_a - n_b}{\frac{1}{\lambda_a^2} - \frac{1}{\lambda_b^2}}$$

The general conclusion arrived at from the experiments upon the alcohols is that "the dispersive powers are continuous functions of the molecular weight, and they increase with increasing molecular weight in the fatty series, and decrease with ascending molecular weight in the aromatic series." Similarly as the result of the investigation of the ethers, it was found that "the value of the dispersive power augments with the molecular condensation." It was further remarked that "the values of the dispersive powers of isomeric ethers are practically identical." In illustration of this latter point, which appears to show that the dispersion practically depends alone upon the molecular weight of the substance, it may be noted that ordinary ether, ethyl oxide, and the isomeric mixed ether, methyl propyl oxide, both possess the same dispersive power, as do also propyl ether, ethyl isobutyl ether, and methyl isoamyl ether. It was also curiously observed that in the mixed ethers of the series $\begin{matrix}(C_nH_{2n-1})\\(C_nH_{2n+1})\end{matrix}\Big\rangle O$, in which the unsaturated allyl radicals occur, the specific dispersive power is approximately the same for all the members of the series, while for the mixed ethers of the series $\begin{matrix}(C_nH_{2n-7})\\(C_nH_{2n+1})\end{matrix}\Big\rangle O$, in which the still further unsaturated aromatic radicles are introduced, the value actually diminishes as the molecular weight increases. The very important conclusion was also arrived at that the specific molecular dispersive power $\frac{B}{d}M$, where M represents molecular weight, of a compound is equal to the algebraic sum of the specific molecular dispersive powers of its constituents. Precisely analogous results were obtained from the experiments upon the liquid fatty acids, confirming the main generalization that dispersion is a true function of the molecular weight.

The additions to the Zoological Society's Gardens during the past week include a Barbary Ape (*Macacus inuus* ♂) from North Africa, presented by Madame Ruoy; a White-fronted Capuchin (*Cebus albifrons* ♂) from South America, presented by Mrs. Akers-Douglas; a Pinche Monkey (*Midis œdipus* ♀) from New Granada, presented by Mr. J. Barry O'Callighan; a Himalayan Bear (*Ursus tibetanus* ♀) from Beloochistan, presented by Mr. B. T. Ffinch, C.M.Z.S.; a Common Raccoon (*Procyon lotor*) from North America, presented by Mr. C. E. Brewerton; a Greater White-crested Cockatoo (*Cacatua cristata*) from Moluccas, presented by Mr. C. J. Cassirer; a Blue and Yellow Macaw (*Ara ararauna*) from Brazil, presented by Mr. A. Cohen; a Pennant's Parrakeet (*Platycercus pennanti*) from Australia, presented by Mrs. Moon; a Water Rail (*Rallus aquaticus*), British, presented by Mr. T. E. Gunn; two Alligators (*Alligator mississippiensis*) from Florida, presented by Mr. Henry Birkbeck; two —— Snakes (*Pituophis melanoleucus*) from New Jersey, U.S.A., presented by Mr. Morton Middleton; two Snow Buntings (*Plectophanes nivalis*), European, purchased; a Vulpine Phalanger (*Phalangista vulpina*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

The Photography of Solar Prominences.—Mr. G. E. Hale, of Kenwood Physical Observatory, Chicago, describes in *Astronomische Nachrichten*, No. 3006, the results of some attempts made last winter to photograph solar prominences. One of the methods employed is to alter the rate of the driving clock of the equatorial, so that a prominence may move slowly across the slit of a spectroscope adjusted radially on the sun's image. A prominence line, say C, is brought into the centre of the field of the observing spectroscope, and at the same time falls upon a photographic plate having a motion such that the radius of curvature of the sun's limb upon it is the same as that of the focus of the equatorial.

A second method proposed is to use a stationary solar image and photographic plate, with two slits in uniform motion—one the radial slit of a spectroscope, the other a slit moving directly in front of the plate.

The plates have been stained with cyanin, alizarin, and erythrosin for the photography of the prominence lines C and D_3, utilized in the experiments. The work is to be resumed shortly with a rotating cylinder, having upon its circumference a strip of dyed celluloid film moving across the focus of the observing telescope, instead of the plates previously used. If this be done, and a uniform motion given by a good clock or clepsydra, some definite results may be expected.

The Frequency of Meteors.—MM. Terby and Van Lint made some observations of the relative frequency of meteors on August 9 and 10, 1890 (*Bulletin de l'Académie Royal des Sciences, Brussels*, No. 9, 1890). An inspection of the observations tabulated shows that before midnight an observer whose field of view embraced one fifth of the horizon, saw from three to five meteors in fifteen minutes. This number was increased after midnight, however, so that an observer, viewing one-sixth of the sky, saw from five to six meteors in the same interval. On August 10 a maximum appeared to be reached between 12h. 30m. and 13h. 11m., the average in that interval being one in two minutes. Only two meteors were observed whose brilliancy was comparable with that of Jupiter. The trains were few, and of short duration.

THE ANNIVERSARY OF THE ROYAL SOCIETY. [1]

THE proceedings of every anniversary meeting remind us of the losses which the Society has sustained during the preceding year by the deaths of several of its Fellows. Of

[1] Presidential address delivered by Sir George Stokes at the anniversary meeting, December 1.

those whom we have lost during the past year, some were distinguished for their scientific labours, some were well known in the world at large. Of several, full obituary notices have appeared or will appear in the Proceedings, but it will, I think, be in accordance with the wishes of the Fellows that I should say a few words on the present occasion about some of those whom we have lost.

The Rev. Stephen J. Perry, S. J., whose name is well-known in connexion with his accurate magnetic observations, and his labours in the domain of solar physics, was last year elected a member of the Council. It was known that he could not attend the first meeting, for he would be on his way to the West Indies to observe the total solar eclipse of the 22nd of December, but we looked forward to having him at the meetings of our Council after that was over. In this we were doomed to disappointment, for a telegram which arrived shortly after the day of the eclipse brought the sad intelligence of his death. His illness began shortly before the day of the eclipse, but he did not suffer it to interrupt his preparations, but worked on to the end as far as his failing strength would allow. The observation of total solar eclipses was a branch of solar physics to which he had paid special attention, and, considering the circumstances of his death, we may regard him as a martyr to science, while at the same time his kindly disposition ensured attachment from all who knew him.

William Lewis Ferdinand Fischer, who was a friend of mine for more than forty years, was a German by birth, and was educated in that country. He came to Cambridge later in life than most men commence residence, entering at my own college, Pembroke. At the time when he entered he was only imperfectly acquainted with the English language. He took his degree in due course, having come out fourth wrangler. He obtained a fellowship at Clare College, and, after some time, was elected Professor of Natural Philosophy in the University of St. Andrews, at which town he continued to reside till his death, and his widow still resides. He was remarkably well acquainted with what had been done in physical science.

In the middle of January we lost, in Lord Napier of Magdala, a man of world-wide reputation, who had been a Fellow of the Society for more than twenty-three years. A sketch of his life has already appeared in the Proceedings.

The eminent physician Sir William Gull, who for some considerable time had been in failing health, was taken from us at the beginning of this year. The Fellows have already in their hands a sketch of his life.

In February there died, at an advanced age, our Fellow Sir Robert Kane, who was eminent among what we may regard as the last generation of chemists. A biographical notice of him is already in the hands of the Fellows.

Robert William Mylne, who died in July, was twelfth in direct descent of a family of architects and engineers, his direct ancestor having had the erection of new buildings for Holyrood Palace in the reign of Charles the Second. He had still in his possession the correspondence relating to that work. His own work lay chiefly in hydraulic engineering and the geology of the south-east of England, especially the London area, and in advising respecting the construction of new buildings in the City.

General Sir John Henry Lefroy, who died last April, combined the duties of his profession and of his responsible offices as Governor of Bermuda, and, for a time, of Tasmania, with active scientific work in relation especially to terrestrial magnetism, with which he was connected by his post as director of magnetic observations at St. Helena and at Toronto. He is the author of a treatise on the subject, and has entered into some investigations bearing on a possible cause of local magnetic irregularities, which seem well deserving of consideration.

Sir Warington Wilkinson Smyth, who was so high an authority on all that relates to mining, and geology as bearing upon it, was one of our Fellows who repeatedly served on our Council and on various committees, and by his sound judgment aided us in our deliberations. Though his health had been failing for some time, his end came upon us with startling suddenness. It will be remembered by many of the Fellows that he was present at our *conversazione* on 18 June, and next morning he breathed his last. He was widely known as a man of science, and was honorary Fellow of various societies on the Continent. In him I have lost one who was formerly a colleague of my own as lecturer in the School of Mines.

William Kitchen Parker held a very high place among biologists in relation especially to the homologies of the vertebrate skeleton. Notwithstanding the laborious nature of his profession, he managed to find time for his scientific pursuits, and our Transactions contain a large number of papers which came from his pen, and are illustrated by elaborate drawings. So highly were his biological researches thought of, that for several years means were found, through an application of a portion of the Government Grant, for enabling him to dispense with the laborious work of his profession, and devote himself to science. His genial disposition and vivacity of manner, and, curiously enough, his personal appearance, reminded one of Faraday.

Dr. James Matthews Duncan, who died suddenly from heart disease, was eminent as an obstetric physician, and was a man of singular straightforwardness of character.

Our Fellow Charles Handfield Jones died on September 30. His chief scientific labours lay in the domain of pathological anatomy.

Alexander John Ellis, who died on October 28, devoted great attention to philology and to the theory of the perfection of musical sounds, and translated v. Helmholtz's work, "Tonempfindungen." He had, shortly before his death, received an honorary doctor's degree from the University of Cambridge.

During the last year we have lost two out of our three senior Fellows, Christopher Rice Mansel Talbot, a Fellow also of the Linnæan Society, who was elected our Fellow in 1831, and, still more recently, in fact only a few days ago, Sir John Francis Davis, who was elected as long ago as 1822, and died at the very advanced age of ninety-six.

The number of Fellows elected before 1847 is now reduced to eighteen, so that in any statistical calculations of the effect of the statutes of 1847 on the number of Fellows, the present condition of the Society may be taken as practically normal.

The Committee appointed in 1888 to consider the best mode of administrating the fund, which was inaugurated in 1882, for founding a memorial to our late eminent Fellow, Charles Darwin, have now presented their report, which has been adopted by the Council. It has been decided that the proceeds of the Darwin Fund be for the present applied biennially in reward of work of acknowledged distinction (especially in biology) in the field in which Mr. Darwin himself laboured; that the award consist of a medal in silver or bronze, accompanied by a grant of £100; that it be made either to a British subject or to a foreigner, and without distinction of sex; and that the award should be conferred at the same time as other medals at the anniversary.

It was further intended, in accordance with Mr. Darwin's known views, that, as a rule, the award should be made rather for the work of younger men in the early part of their career than as a reward to men whose scientific career is nearly finished.

The Committee appointed at a meeting of the Council held immediately before the last anniversary meeting of the Society, to set on foot a memorial of our late Fellow James Prescott Joule, have naturally not got quite so far in their work. They decided that the memorial should take an international character, and should have for its object the encouragement of research in physical science, and should also have in view the erection of some personal memorial in London. The subject was accordingly brought to the notice of a number of scientific men abroad, from many of whom favourable replies have been received. The Joule Committee have resolved, "That the balance of the fund, after providing a suitable personal memorial, be transferred to the President, Council, and Fellows of the Royal Society, and that the President and Council be requested to undertake the administration of the proceeds in such manner as may appear to them most suitable for the encouragement of research, both in England and abroad, especially among younger men, in those branches of physical science more immediately connected with Joule's work;" and, also, "That the treasurer be instructed to retain for the present a sum not exceeding £300 for the expenses of the medallion, and hand over the balance to the President, Council, and Fellows of the Royal Society."

This offer of the Joule Committee was accepted with thanks by the Council, but the further consideration of the steps to be taken has not yet been entered on. Meanwhile the treasurer of the fund has handed over to the treasurer of the Royal Society a sum of about £1,400.

In 1663, when the second charter was granted to the Society, a body of statutes was drawn up for regulating various matters not fixed by charter. Alterations have since been made from

time to time, as provided for in the statutes themselves. The last considerable alteration was made in 1847, when the present system was introduced, according to which the Council select from the candidates, other than those who have a special privilege as to coming on for ballot, a definite number whom they recommend to the Society for election, and the election takes place on one definite day in the year. A few changes, of less importance, have been made since that time, and experience has pointed out the desirability of some changes of detail, chiefly as regards the mode of dealing with papers. A committee was appointed last session, and continued at the commencement of the present, to revise the whole body of statutes, with a view to bring them into stricter conformity with existing practice, and at the same time to propose further changes, should any such appear desirable. The Committee have now reported; but as the session was near its end, and the subject was one requiring full consideration by the Council, the report has been merely received and entered on the minutes, and it has been left to the Council that is to be elected to-day to take such further steps as may appear to them desirable.

Some of the proposed alterations relate to the mode of dealing with papers which are communicated to the Society, which is a matter of practical business that may well be left to the judgment and experience of the executive body. But some points have been raised which it seems desirable to bring to the notice of the Fellows at large, in order that they may have an opportunity of considering them before a final decision is come to by the new Council.

The question has more than once been raised whether, considering the increase of population and the more general diffusion of scientific knowledge which has taken place within the last forty years, the number of candidates to be selected by the Council for recommendation to the Fellows for election might not now, with advantage, be made a little larger than fifteen, the number at which it was fixed in 1847. On this question there was considerable difference of opinion in the Committee, but the majority were in favour of keeping the number as it is at present.

Connected, to a slight extent, with this question is another, whether the Council should not have the power of recommending to the Fellows for election, in addition to the fifteen selected from among the candidates on the ground of scientific merit, a strictly limited number of men of very high eminence in other ways. The Committee recommend that such a power be entrusted to the Council, the number of Fellows who have been thus elected, existing at any time, being limited to a maximum of twenty-five, and the number elected in any year to a maximum of two.

The question was also discussed whether the maximum number of foreign members, which at present stands at fifty, should be increased, and was decided in the negative.

Another recommendation of the Committee which perhaps it may be as well to mention, is one enabling the Council, in any year, to regulate for the ensuing year the length of the Christmas and Easter holidays. At present the weekly meetings are resumed in the second week after Christmas week, and there is then no intermission till Passion week, though the earlier portion at least of this interval is a time during which papers intended for reading do not usually come in so frequently as towards the end of the session. According to the statute in force till 1888, three of the ordinary weekly meetings between Whit-Sunday and the last meeting in June, were cut out by the Whitsun holiday, Ascension Day, and the annual election of Fellows; and as at that time of year papers commonly come in pretty frequently, there was a considerable congestion of papers towards the close of the session. This congestion was partially relieved by an alteration of the statutes, which came into force in 1888, enacting that an ordinary meeting should be held at the conclusion of the Annual Meeting for the election of Fellows; but the fact that the proportion between the number of meetings held and the number of papers that come in varies a good deal with the season, seems to render it desirable that the regulation of the number of meetings should be rather more elastic, and should to some extent be left in the hands of the Council.

Since the last anniversary twenty-five memoirs have been published in the Philosophical Transactions, containing a total of 1068 pages and 72 plates. Of the Proceedings, eleven numbers have been issued, containing 1165 pages.

In the library, the work of making room for growing series, and of obtaining volumes or parts to complete series that were imperfect, has been continued. In the course of this work the Council have, upon the recommendation of the Library Committee, distributed some 1500 volumes, consisting partly of duplicates and partly of works of small scientific value, among various public institutions. The catalogue of the manuscripts, which I mentioned in my last year's address, as about to be commenced, has been completed during the past session. The maps and charts, the pictures and busts, have also been catalogued, and the collection of the manuscripts of memoirs in the Philosophical Transactions, the Proceedings, and the Archives, having been completed, the binding of them is now going forward.

With a view to increasing the circulation of the Society's publications on the Continent of Europe, Messrs. Friedländer and Son, of Berlin, have lately been appointed additional agents for their sale.

The Royal Society has always been ready to assist the Government of the day when requested so to do, by expressing its opinions or offering its advice on questions involving special scientific knowledge. Last year I received, as your President, a request from the President of the Board of Trade that I should, in conjunction with two Fellows of the Royal Society nominated by me in consultation with the Council, examine a report in two parts presented by the Corporation of the Trinity House to the Board of Trade, relative to lighthouse illuminants, and express our opinion whether the conclusions of the Trinity House, as set forth in their Reports, are justified by the records of the experiments contained therein. Lord Rayleigh and Sir William Thomson were asked, and consented to join me as referees, and our Report was some time ago sent in to the Board of Trade.

Another subject in which scientific principles are blended with practical application, is that of colour blindness in its relation to the correct perception of coloured signals used at sea and in the railway service. It is easy to understand what serious accidents might be occasioned, for instance, by confusing red with green; and so well is the liability to such confusion, arising from a not very rare abnormal condition of colour perception, understood at the present day, that persons who propose to engage in service at sea or on the railways are now, as a matter of course, examined as to their perception of colour. But, glaring as the difference between red and green appears to persons whose vision is normal, the detection of those who are liable to confound them, and who, for the most part, are quite unconscious that they see colours differently from people in general, is by no means so easy as it might appear at first sight; and there appeared reason to think that sometimes the tests applied are defective, and let pass persons who are afflicted with this peculiarity of vision, while, on the other hand, they may lead to rejection of persons whose vision is normal, perhaps, after they have engaged in their course for life in an employment for which normal vision is demanded. Mr. R. Brudenell Carter wrote a letter to us suggesting that we should appoint a committee to investigate the subject of colour blindness, and after discussion of this proposal I was requested to write a letter to the President of the Board of Trade informing him that, should the Government desire it, the Council will be prepared to appoint a committee to consider the whole question of colour blindness. A reply was received from the Board thanking us for the communication, and saying that they regarded with satisfaction the proposal of the Council to appoint such a committee. A committee has accordingly been appointed, and has held several meetings, and examined several witnesses; but the subject is a wide one, and the committee have not yet brought their labours to a close.

The proceedings of to-day bring to an end my long tenure of office in the Royal Society, which has extended now over thirty-six years, during the last five of which I have held the honourable office of your President. I am deeply sensible of the kindness which I have always experienced from the Fellows, and of the indulgence with which they have overlooked my deficiencies, due, in part, to the pressure of other work. It cannot be without a strong feeling of regret that I come to the close of an official connection with the Society that has now extended over full half my life. But I feel that it is time that I should make way for others, and that I should not wait for those infirmities which advancing years so often bring in their train; besides which, there are personal reasons which led me to request the members of the Council not to vote for my nomination for re-election as your President.

And now it only remains to me, as virtually my last official

act as your President, to perform the pleasing duty of delivering the medals which the Society has to award to the respective recipients of those honours.

The Copley Medal has been awarded to our Foreign Member, Prof. Simon Newcomb, who has been engaged during the last thirty years in a series of important researches, which have contributed greatly to the progress of gravitational astronomy. Among his labours in this field may be mentioned his able discussion of the mutual relations of the orbits of the asteroids, with reference to Olbers' hypothesis, that they were formed by the breaking up of a ring of nebulous matter, his discussion of the orbits of Uranus and Neptune, and of the orbit of the moon. Recently he has turned his attention to Saturn's satellites, and has investigated the remarkable action of Titan on Hyperion. For many years back he has chiefly been engaged in perfecting the tables of the moon; and in his important work, "Researches on the Motion of the Moon," he has discussed observations of eclipses and occultations previous to 1750, with the important practical result that, by the removal of an empirical term of long period from Hansen's lunar tables, and by an empirical alteration of another term of long period, he is enabled to represent satisfactorily the observations of the moon from 1625 to the present time; and by thus indicating empirically the long period inequality required to represent the moon's motion, he has prepared the way for its theoretical investigation.

The Rumford Medal has been awarded to Prof. H. Hertz for his work on electro-magnetic radiation.

One of the most remarkable achievements of the late Prof. Clerk Maxwell was his electro-magnetic theory of light, in which it was shown that a certain velocity, determinable numerically by purely electrical experiments, and expressing theoretically the velocity of propagation of an electro-magnetic disturbance, agreed within the limits of error of experiment with the known velocity of propagation of light; and accordingly that we have strong reason for believing that light is an electro-magnetic phenomenon, whatever the appropriate physical idea may hereafter prove to be which we ought to attach to the propagation of an electro-magnetic disturbance. But as yet no means existed by which phenomena, such as those of interference, which are bound up with the propagation of undulations, could be exhibited by purely electrical means. Prof. Hertz was the first to detect electro-magnetic waves in free space by his invention of a suitable receiver, consisting of a resonating circuit, which gives visible sparks when immersed in a region of sufficiently intense electric radiation.

By reflection, refraction, and interference experiments, he has further verified the undulatory nature of the disturbance near a quick electric oscillator, such as had been suggested by Prof. Fitzgerald, on the basis of Clerk Maxwell's electro-magnetic theory of light, and Sir W. Thomson's theory of the oscillatory character of a Leyden jar discharge.

These important researches contribute powerfully to the inducements we have to refer the phenomena of light and electricity to a common cause, different as hitherto their manifestations have been; and by this means the theory of each may be advanced through what we know of the other.

One of the Royal medals has been awarded to our Fellow, Dr. David Ferrier, for his researches on the localization of cerebral functions.

We owe to his experiments, and the method of experimenting upon monkeys which he introduced, almost the whole of our knowledge of cerebral localization in man. From pathological observations, Broca located the centre for speech in the third left frontal convolution, but with this exception nothing was known of cerebral localization in man until Dr. Ferrier commenced his experiments in 1873.

Fritsch and Hitzig, in 1870, had observed that definite movements could be obtained by electrical irritation of the cerebral cortex in the dog, and this indicated the existence of localized motor areas in the brain. They did nothing, however, towards localizing sensory centres, and even in regard to the motor centres their observations were very limited. Their observations attracted hardly any attention; they had neither followed them up themselves, nor had any one else taken them up; and when Dr. Ferrier began his experiments he was ignorant of their observations, and discovered the method independently. By the happy device of experimenting upon monkeys, whose brains present a great similarity in the arrangement of the convolutions to those of man, he was able to map out the most important *motor* areas with great precision; but not content with the investigation of motor centres, he experimented on the localization of *sensory* centres in the brain, and not only showed conclusively the existence of such centres, but determined their position. In addition to his work on cerebral localization he has shown, in conjunction with Dr. Yeo, that the complex, and at first sight seemingly purposeless connections of nerve fibres in certain plexuses is really connected with the co-ordination of various muscles for definite purposive movements. As so often happens, these researches, purely scientific in the first instance, have been turned to practical account. Dr. Ferrier himself predicted the application of cerebral localization to cerebral surgery. This application he and others have already made, and his prediction is now being fulfilled with brilliant results.

The other Royal Medal has been awarded to our Fellow, Dr. John Hopkinson, for his researches in magnetism and electricity.

Dr. Hopkinson's researches in magnetism comprise investigations of the effect of temperature upon the magnetic properties of iron, nickel, and various alloys of these metals (Phil. Trans., 1889, A, p. 443). Before these investigations were published it was thought that increased temperature tended to diminish the magnetic susceptibility of iron. Dr. Hopkinson's experiments show, however, that, on the contrary, the magnetic susceptibility increases enormously as the temperature increases, until the temperature reaches about 660° C.; beyond this temperature iron entirely ceases to be magnetic.

Dr. Hopkinson's contributions to the theory of dynamo-electric machinery are most important. The method, now so extensively used, of solving problems relating to dynamos by the use of what M. Deprez has called the "characteristic curve," is due to him.

He has also made a series of determinations of the specific inductive capacities and refractive indices of a large number of transparent dielectrics, the results of which are of great importance in the theories of electricity and light.

The Davy Medal has been awarded to Prof. Emil Fischer for his discoveries in organic chemistry, and especially for his researches on the carbo-hydrates.

To him, in conjunction with Otto Fischer, we owe the determination of the constitution of rosaniline, a most valuable dye-stuff, and the typical member of a very large group of important dyes.

He is the discoverer of phenylhydrazine, one of the most important of the reagents placed at chemists' disposal within recent years, and he has most exhaustively studied the behaviour of this substance and its congeners. The hydrazines have also been employed by Fischer in preparing indole derivatives, among others, skatole, and the study of a class of substances of considerable physiological importance has thereby been rendered possible.

During the past seven years Fischer has devoted his attention to the study of the sugars, and has obtained most marvellous results, having succeeded in preparing, by purely artificial methods, the known sugars dextrose and levulose, as well as other isomeric sugars, and having established the relationship of the various members of the glucose group. He has, in addition, determined the constitution of milk-sugar and of starch-sugar—the isomer of cane-sugar formed on hydrolysing starch. He has also prepared "glucoses" containing seven, eight, and nine atoms of carbon, and has established the remarkable fact that only those which contain three, six, or nine atoms of carbon are fermentable by yeast. His researches are not only of the highest value to chemists, but also of extreme importance to physiologists, on account of the insight which they afford of the processes concerned in the natural formation of sugars.

The Darwin Medal has been awarded to Mr. Alfred Russel Wallace for his independent origination of the theory of the origin of species by natural selection.

It was natural that this, the first award of the Darwin Medal, should have been made to one who independently originated the theory, since named that of natural selection, which, in conjunction with his other numerous and important contributions in the domain of natural history, have made the name of Darwin so famous, and who made known a large series of important and novel observations in support of that theory, the result of many years work in the Malay Archipelago. These views Mr. Wallace has subsequently most ably advocated in various published works, among others his laborious volumes on the "Geographical Distribution of Animals," his brilliant "Island Life," and more recently his "Darwinism," which was published only last year.

WHO ARE THE AMERICAN INDIANS?

ATTENTION has once more been attracted to the American aborigines by the troubles of the United States Government with some of the Indians of Dakota and various neighbouring districts. Special interest therefore attaches to a lecture on the question "Who are the American Indians?" recently delivered by Mr. H. W. Henshaw, in the National Museum, Washington, in the "Saturday Course," under the auspices of the Anthropological, Biological, Chemical, National, Geographic, and Philosophical Societies. The following is the more important part of the lecture:—

When Columbus discovered America he discovered not only a new continent but a new people—the American Indians. From one end to the other of its broad expanse the continent was occupied by Indian tribes that had held the land from time immemorial—so far at least as their own traditions aver—knowing nothing of any country but their own. The commonly presented picture of the Indians as they appeared at the time of the discovery is that of a horde of wandering savages, half or wholly naked, living on roots and herbs, or existing by the capture of wild animals scarcely more savage than themselves, and the chief objects of whose existence was to enslave, to torture, and to kill each other. Those who hold such opinions have ever taken a hopeless view of the Indian's present and a still more hopeless view of his future. Such a picture conveys a totally false impression of the Indian and of the state of culture to which he had attained at the era of the discovery. Though still living in savagery, he was in the upper confines of that estate, and was fast pressing upon the second stage of progress, that of barbarism—that is to say, he had progressed far beyond and above the lowest states in which man is known to live, to say nothing of the still lower conditions from which he must have emerged, and had travelled many steps along the long and difficult road to civilization.

Already he had become skilful in the practice of many arts. Though the skins of beasts furnished a large part of his clothing, he had possessed himself of the weaver's art, and from the hair of many animals, from the down of birds, and from the fibres of many plants he knew how to spin, to weave, and to dye fabrics.

Basket-making he had carried to so high a degree of perfection that little further improvement was possible.

The potter's art also was his, and though his methods were crude and laborious, the results achieved, both as regards grace of form and ornamentation, may well excite admiration at the present day.

Copper had been discovered and was mined and roughly beaten into shape to serve for ornament and, to some slight extent, for mechanical use. In Mexico and Peru, gold, silver, and copper were worked, and many authors contend that the method of making bronze, an invention fraught with tremendous possibilities, had there been discovered.

In much of South and Central America, Mexico, and the eastern parts of the United States, so important an advance had been made in agriculture that it furnished a very large part of the food supply, and it should not be forgotten that the chief product of the Indian's tillage, maize or Indian corn, which to-day furnishes a large part of the world's food, was the gift of the Indian to civilization. A scarcely less important contribution to mankind is the potato, the cultivation of which also originated with the Indians. A third important agricultural product, though less beneficial, is tobacco, the use and cultivation of which had been discovered centuries before the advent of the European.

Architecture may seem like a large word to apply to the dwellings of the Indians. Nevertheless many of their houses were more substantial and comfortable than is generally supposed, while in the North-West many tribes reared dwellings of hewn planks, sometimes as large as 210 feet long by 30 feet wide, which were capable of accommodating several hundred individuals. More pretentious and durable were the communal houses of mud and stone reared by the Pueblo people of Arizona, New Mexico, and Mexico, while further south, in Central and South America, were edifices of hewn stone, which from their dimensions, the size of some of the blocks contained in them, and the extent and ornate character of the ornamentation, justly excite the wonder and admiration of the traveller and archæologist.

The advantages of a beast of burden had been perceived, and though the human back furnished by far the greater part of the transportation, yet in North America the dog had been trained into an effective ally, and in the Andes the llama performed a similar office. Insignificant as was the use of the dog as a carrier, its employment cannot well be over-estimated as a step in progress when it is remembered that the plain's tribes that most employed it lived in the midst of the buffalo, an animal which must have become of prime domestic importance in the never-to-be-enacted future of the Indian.

The need of some method of recording events and communicating ideas had been felt, and had given rise, even among the ruder tribes, to picture-writing, which in Mexico and Central America had been so far developed into ideographs, popularly called hieroglyphics, as to hint strongly at the next stage, the invention of a true phonetic alphabet; nay more, the Mexicans and Mayas are believed to have reached a stage of true phonetic writing, where characters were made to represent not things, as true ideographic writing, but the names of things and even of abstract ideas; and this is a stage which may be said to be on the very threshold of one of the proudest achievements of civilization, that of a phonetic alphabet.

Instead of living in an unorganized state, where each man was a law unto himself in all things, the Indians lived under organized forms of government, rude enough indeed when compared with the highly organized system of civilized nations, but marking an essential advance on the conditions attained by savage peoples in other parts of the world. The chieftaincy was transmitted by well understood laws, or, as in some tribes, was more purely elective. Their social system was very ingenious and complex, and, being based largely upon kinship ties, was singularly well fitted for the state to which they had attained, of which indeed it was simply an expression and outgrowth. In many sections a considerable advance had been made in political confederation, and neighbouring tribes combined for defence and to wage war against a common enemy. They had invented many and singularly efficient laws to repress and punish lawlessness against the individual and the social body, and as a consequence they enjoyed almost entire immunity from theft and many other crimes.

The development of religious ideas among our Indians is a curious and instructive study. Though the Great Spirit and the Happy Hunting Ground which missionaries and theologians thought they had discovered among them are now known to have had no existence, the Indians had by no means reached the state of culture in which they were found without developing religions. Their gods or fetishes were innumerable, their priests endowed with immense influence, and their ceremonies of devotion and propitiation were as devout as they were elaborate. The precision of the beliefs of many tribes and the elaborateness of their rituals are simply astonishing. Thus their advance in the domain of religious thought equalled, if it did not surpass, their progress in some other directions.

If by medicine we mean the rational treatment of disease, the Indian can be said to have learned only the rudiments of the healing art. Medicine, in so far as it was a distinct profession, was almost wholly in the hands of the Medicine Man or Shaman, who filled the twofold office of priest and doctor. Neither the theory nor the practice of the Shaman had in it anything that was rational and very little that was efficacious, except through the influence exercised over the mind of the patient—in other words, except so far as the Shaman was a faith-curer. Whatever that is marvellous in the modern cases of faith-cure can be more than matched out of the practice and experience of the Shaman, who learned his trade long before the European came to these shores. He who would see the Indian Shaman need not seek the wilds of the Far West. He may find his counterpart on Pennsylvania Avenue. The whole medical practice of the Indian Shaman was based upon the idea that all disease was the effect of evil disease spirits that had obtained lodgment in the body, or that it was caused by witchcraft, and so long as practice was directed to the dislodgment of these spirits no rational treatment was possible. I am aware that the above idea of Indian medicine is contrary to popular belief, which to some extent at least is in harmony with the claims of alleged Indian doctors of white extraction who claim to have derived their skill and their herbs directly from the hands of Indian experts. Recent and carefully conducted investigations on this subject, however, fully substantiate the above statements. Though roots and herbs were employed in the treatment of nearly all diseases, they were chiefly used as adjuncts to the

charms and sorceries of the medicine man. Often they were not given to the patient at all, but were taken by the medicine man to heighten his power over the disease spirits. Often they were applied by being rubbed on the body of the patient or by being blown in the shape of smoke on the afflicted part.

Among the Indians was found flourishing to a remarkable degree the so-called doctrine of seals or signatures. A few examples of the doctrine derived from the eastern Cheroki by Mr. James Mooney may prove of interest. Doubtless you are all familiar with the cone flower. The Cheroki call it deer eye, and from its fancied resemblance to the strong-sighted eye of the deer and its connection by name, for the Indian believes that there is a potent connection between the name of a thing and the thing itself, it is used as a wash for ailing eyes.

The common purslane (*Portulaca oleracea*) is used as a vermifuge, because the red stalk looks like a worm.

An infusion of the roots of the hoary pea (*Tephrosia virginiana*), called devil's shoe-strings in the South because of their toughness, is used by the Cheroki ball-players as a wash to strengthen their bodies, and by the women as a hair wash to strengthen it and keep it from falling.

Who of you has ever walked in our woods without getting on his clothing the common beggar's lice (*Desmodium*)? How tenaciously they stick you all know; so do the Cheroki, and because the burrs stick fast they use a tea made of them to strengthen the memory. The Cheroki at least can dispense with the services of a Loisette.

You whose ambition it is to be good singers have only to drink a tea of crickets, according to the Cheroki, for does not the cricket possess a fine voice and doth he not sing merrily?

The tendency of the human mind to speculate and to draw inferences—a tendency common alike to the savage and the civilized man—cannot be held in check forever, however strong the bonds; and just as knowledge and science escaped from priestly thrall within the history of civilized times, so a certain small amount of knowledge of the therapeutical use of drugs was gaining ground among the common folk of the Indians. It was fairly to be called old women's practice, as it was largely in their hands. It grew out of observation; infusions of certain herbs produced certain results, acted as emetics or purgatives, and hence these herbs came to be employed with something like an intelligent purpose. Many of the herbs used were absolutely inert; many were harmful, of course, since where there is practically no true diagnosis and no correct knowledge of the effect of drugs there can be no really intelligent selection of remedies; but in the case of certain simple diseases, herbs, the actual cautery, and above all the sweating process, were beginning to be recognized by the common folk as serviceable, and to be employed to some extent without recourse to the Shaman.

As the child must creep ere it can walk, in such theories and treatment, childish though they may seem, may be discerned the beginnings of the noble science of Medicine, which, having largely cast aside the superstitions that hampered her infant steps, now walks erect, and although of late she seems to have revived the beliefs of her childhood, her handmaiden, Science, bids her call the demon disease spirits ignorance and vicious habits, the diseases themselves bacilli or germs. The Indian believes that the white man carried the spirit of small-pox in bottles, and let it loose among them. Modern science actually does bottle the small-pox germs, and germinates them at will. So the Indian theory of disease reappears in a new form.

Such in briefest outline are some of the achievements of the Indian as he was found by civilized man. Whatever value may be placed upon them, whatever rank may be assigned them in the scale of human efforts, they were at least his own, and some of them compare favourably with the record of our Aryan ancestors before they split up into the numerous nations which have done so much to civilize the world. Many, I am aware, hold that the Indian had progressed as far towards civilization as his capacities admitted; others have held, and possibly some now hold, that he was already on the decline; they see in his crude ideas and rude inventions only the degradation of a higher estate; in other words, instead of a savage preparing to enter civilization through the necessary half-way state of barbarism, he is held a half-civilized man lapsing into savagery. Such views, it is needless to say, find no favour in the mind of the evolutionist. To him, the achievements of the Indian are only the milestones which have marked the progress of every civilized nation in its march from what it was to what it is; to him the chief value and significance of his studies of the mental state of the Indian, as expressed in his mythology, his medicine, his social and political organization, or in his more concrete arts, is the fact that in them he reads the records of his own past. If there be any truth whatever in the theory of evolution as applied to human progress, only one inference can be drawn from the history of the Indian race as it appears in historical pages and in the no less eloquent records interpreted by archæologists. This inference is that, starting in its career later than some other races, or being less favoured by circumstances or conditions of environment, or possibly being less endowed, the Indian, despite all, had progressed an immense distance towards civilization; that the race contained all the capabilities for a further advance, and for achieving a civilization of its own, differing, it may be, markedly from our own, as other civilizations differ, but still containing within itself all the essentials of that wonderfully complex thing called civilization. Such at least is the lesson evolution teaches.

Hardly had the new land been discovered when the question arose, Who are the Indians and where did they come from? Naturally enough the Indian had his own answers to these questions. As to who they are all tribes agree. "We are men," said the Illinois to the French; and the name of every tribe in America—the name by which they know themselves—signifies "true men," "men of men," "the only men," as evincing their superiority to all others. As to their origin, their ideas are as confused and perplexing as they are multifarious and conflicting. It may almost be said, as many tribes so many origins. A large number of tribes claim to have originated in the localities where they were first found by Europeans, where they emerged from the ground or came from the recesses of some neighbouring mountain. The Chocta, for instance, claim to have come from an artificial mound in Mississippi. The mound has a depression at its centre which is accounted for by the Creator stamping upon it to close the aperture when he saw that a sufficient number had emerged to form the Chocta tribe. One of the Shawni tribes was created from the ashes of the fire. The Yuchi, of Georgia, claim to be children of the sun, who is their mother, and the earth, their father, an exact reversal of the usual parentage. The Pomo, of California, claim that their ancestors, the Coyote men, were created directly from a knoll of red earth. The Klamath, of Oregon, were made from the service berry. The Yokut, of California, emerged from badger holes, as their name implies. Somewhat more poetical is the idea of the Aht, of Vancouver Island, who allege that animals were first created at Cape Flattery, and from the union of these with a star that fell from the skies resulted the first men, their ancestors.

The above are fair examples of the ideas entertained by the Indians respecting their own origin. Puerile they certainly are, yet who will maintain that they are more so than the theories of origin held by the Greeks and other classical peoples?

Who, then, are the American aborigines? For Columbus and his followers there was but one answer to the question. As he had reached the eastern shores of India the people must be Indians, and his error is perpetuated to-day in the name. Later, when the newly discovered country was found to be not an old but a new continent, the question of the origin and consanguinity of the Indians was renewed. So strongly tinged with religious thought was the philosophy of the day that Biblical sources were naturally first appealed to to solve the knotty problem. As mankind was supposed to have originated in Asia, and as all but the ten lost tribes were accounted for, they were rationally appealed to for the origin of the Indian. Perhaps the best exponent of the belief in the Jewish origin of the Indians was Adair, who published his celebrated essay in 1775. Thoroughly familiar with Indian beliefs and customs, he succeeded in bringing together a mass of evidence, derived from a comparison of religious rites, civil and martial customs, marriages, funeral ceremonies, languages, and traditions, as curious and contradictory as it is inconclusive.

The Jewish origin of the Indians secured a very strong hold on the minds of the writers and thinkers of the eighteenth century, and so firmly did the theory take root that it has never been wholly given up, but is held to-day by a greater or less number as the only rational belief.

Though the favourite, the Jewish hypothesis is by no means the only one. Men of science and laymen count their theories by the score. The Bible and ancient philosophy alike have been appealed to in support of pet hypotheses.

One believes America to have been colonized by Phœnician merchants; another, by Carthaginians. America was peopled by Carthaginians, says Venegas, and Anahuac is but another name for Anak. Besides, both nations practised picture-writing; both venerated fire and water, wore skins of animals, pierced the ears, ate dogs, drank to excess, telegraphed by means of fires on hills, wore all their finery on going to war, poisoned their arrows, beat drums and shouted in battle. Not an unfair example this of the scientific deductions of the day. Surely he must be unreasonable who refuses to be convinced by such testimony!

Says the pious Cotton Mather, the natives of the country now possessed by the New Englanders had been forlorn and wretched heathen ever since they first herded here, and though we know not when or how these Indians first became inhabitants of this mighty continent, yet we may guess that probably the devil decoyed those miserable savages hither, in hopes that the Gospel would never come here to disturb his absolute empire over them.

The evidences that the Indians came from Scandinavia are as convincingly put as those proving that they came from Ireland, or Iceland, or Greenland. Equally conclusive are the arguments for a passage by the Indian across Bering Strait from Asia, across the Northern Pacific from Japan or China in junks, or across the Southern Pacific in canoes from the Polynesian Islands, or Australia. Even Africa is not too far distant to send its contingent to the new land; and when the ocean has been deemed to be too broad to permit a passage from foreign shores the willing imagination of the writer has dropped an island into mid-ocean, and called it Atlantis, to facilitate alike the crossing of the Indian and the reception of a fanciful theory. Thus there is a theory of origin to suit the tastes of all. If you have a special bias or predilection, you have only to choose for yourself. If there be any among you who decline to find the ancestors of our Indians among the Jews, Phœnicians, Scandinavians, Irish, Welsh, Carthaginians, Egyptians, or Tartars, then you still have a choice among the Hindu, Malay, Polynesians, Chinese, or Japanese, or, indeed, amongst almost any other of the children of men.

Preposterous as may seem many of the theories above alluded to, nearly all of them rest upon a certain basis of fact and comparison. Many, at least, of the similarities of thought, custom, methods, arts, religions, and myths from which the theories are deduced indeed exist, though false analogies permeate them all. The thread of fact which sustains the theories is, moreover, far too slender to bear the weight put upon it. It is not that the theories contain so much that is erroneous but the proof offered is entirely insufficient. The science of yesterday reared its edifices upon foundations of fact the very slightest. The science of to-day demands broader foundations and more deeply laid upon which to base its conclusions. Erroneous hypotheses like the above have, however, been productive of great good in pointing out and emphasizing some of the most useful lessons which the student of anthropology of the present day must learn and ever keep in mind. Of these perhaps the most important is that the human mind is everywhere pratically the same; that in a similar state of culture man in groping his way along will ever seek the same or similar means to a desired end. That, granting the same conditions of environment, man acts upon them and is acted upon by them in the same way the world over. Hence, in large part, arise those similarities of customs, beliefs, religions, and arts which have been appealed to as evidences of genetic connection or of common origin, when, in fact, they are evidences of nothing but of a common humanity.

This leads us to speak briefly of some of the leading methods of classification by which men of science have sought to solve the problem of the origin and relation of races, and among other peoples of our own Indians.

The physical tests of race most approved by ethnologists are colour, viz. the colour of the skin, hair, and eyes; the structural differences of the hair; the size and shape of the skull as determined by capacity and measurements; and the test of language.

Few of the tests formerly relied upon in classifying mankind have proved less satisfactory to modern investigators than that of colour. The microscope appears to show that colour is not due to organic differences of race; not only are there great differences in the colour of individuals of the same tribe, but of the same family, and even in the same individual at different periods of life. Thus, in the case of our Indians, it i well known that the skin of the infant at an early age is very light-coloured, scarcely distinguishable, in fact, from a Caucasian child, and that it assumes a deeper shade only with advancing years. This, I believe, is true irrespective of tribe or habitat. The hair of the Indian child is brown instead of black. The colour of the adult Indian varies within very wide limits, but singularly enough he is never copper-coloured or red, as he has been called from the time of the discovery. All our Indians are brown, and while certain tribes, as the Tuscaroras and Mandans, are so light as to give rise to the theory that they are descendants of the Welsh, other tribes, as some of the Californians, are so dark as to closely approach the black Africans. I say black Africans, for it is to be remarked, in passing, that some African tribes are very light-coloured.

The division of mankind into four groups—white, black, copper, and olive—is, in a general way, consistent with facts. Moreover, these divisions are, to a certain extent, correlated with geographical range and climate, and thus correspond to the colour differences of birds and animals. That they are also and perhaps more strictly correlated with culture status is not to be doubted; for it may be said, in a general way, that all civilized peoples are light-coloured and nearly all barbaric and savage peoples are dark-coloured. So complete, however, is the intergradation of colour when all varieties of mankind are considered, and so intangible are the distinctions which must be relied upon to distinguish them in the case of individuals and even of tribes, that it appears that while colour affords a convenient off-hand means of classification, and while it may be made useful in connection with other criteria, it is quite insufficient in itself as a test of race.

The more obvious peculiarities of the hair, according as it is straight, crisped, or curly, early attracted the attention of ethnologists. The modern microscope has shown that these peculiarities are more or less intimately correlated with structural differences, and that the straight hair of the American Indian and the Mongolian is nearly cylindrical in section, while the frizzled hair of the Negro and Papuan shows an oval or flattened section. Between the two extremes, however, are too many shades of difference to permit the extensive use of this criterion as a race classifier, except in a subordinate way.

Much time and thought has been expended by craniologists in the effort to classify races by means of the skull. Notwithstanding the great ingenuity exercised in devising methods and instruments to secure constant results and trustworthy figures as a basis for comparison, the results thus far obtained have been disappointing. So faulty were the mechanical means adopted by the earlier craniologists that students of to-day are compelled to discard their data and resulting conclusions and begin almost *de novo*. There are many able men who are sanguine not only that the physical structure of man may yet be made to reveal secrets bearing upon the origin of races, if there be more than one, but that the science of craniology in particular is destined to have an important bearing upon these racial problems. Whatever the future of craniology or the other methods of classification by physical characteristics may have in store, the contradictory results thus far obtained offer little to satisfy us. Not only do the naturalists and ethnologists who have studied man's physical characteristics differ as to the number of races of mankind, and as to what constitutes a proper basis for classifying them, but thus far there has been little agreement as to the assignment of particular tribes or peoples. Perhaps more authorities are agreed that there is but one race and one origin of mankind than agree upon any greater specific number; but when it is remembered that there are authorities who place the number of distinct races at two, three, four, five, six, seven, eight, eleven, sixteen, and that one places the total as high as sixty-three, it will be agreed, I think, that it is better to suspend judgment and not to accept any present result as final.

We have already noted that the earlier theories of origin for the Indian, based as they largely were upon certain assumed analogies of customs, laws, religious observances, myths, &c., rested upon such slight foundations as to hardly entitle them to be classed as scientific hypotheses. We have also seen that up to the present time the attempts to classify mankind by his physical characters have produced discordant results, and that little dependence is to be placed upon the results themselves or upon the theories arising therefrom which relate to the more

profound question of the origin of races. In turning to the test of language, if doubt and uncertainty were left behind and harmony and agreement took the place of discordant views we might count ourselves fortunate indeed. But such is not the case. We have indeed only to go back a short time to find that the generalizations drawn from the study of language are as crude, the hypotheses as baseless, the theories as wild as are those we have just glanced at. Nor is this strange. Like its sister sciences, linguistics had to pass through a period in which speculation and hypothesis, instead of going hand-in-hand with facts and induction, usurped their place. Until the inductive method was born no science of philology was possible. The science of comparative philology is indeed of recent origin, nothing worthy of the name existing before the present century. Within the last fifty years it has made a wonderful growth and achieved results little short of marvellous. Though still in its infancy as regards future possibilities, and while it needs and welcomes the aid of all the other sciences to solve the complex questions which come properly within its domain, it is unquestionably our best guide in problems relating to the origin and relationship of the races of mankind. . . .

No part of the known world affords a better opportunity for the study of the nature of language and its processes of growth than America. The Indian languages are by no means the most primitive at present spoken by man, and it may surprise some of my hearers to be told that in respect of some of their characteristics they compare favourably with Greek and other classic tongues, though the classic languages as a whole belong to a much higher stage of development. Instead of being mere jargons of words, disconnected with each other and capable of expressing only the simplest ideas, as I find many intelligent people believe, they are in some directions singularly highly developed, and not only are they capable of serving as the vehicle of every thought possible to their possessors, but their vocabularies are extensive, possess many synonyms, and furnish the means of discriminating the nicest shades of meaning.

There is not a principle or process in the most highly developed languages of which the germs at least are not discernible in Indian languages. The differences are not those of kind but of degree of culture.

Moreover inherent in them is the power of unlimited growth and expansion, and just as our own language grows, keeping step with advances in science and art, so Indian languages are capable of a development equal to the most exacting demands of civilization.

While thus in many respects highly developed, Indian languages are not to be compared as vehicles of thought with such languages as our own English, for instance. As a body they are still in that stage of development in which the various processes of language-making may be studied with comparative ease. Just as the various natural processes by which mountains are levelled and the earth's surface carved out and remodelled are more apparent and more readily studied by the geologist in the still primitive West, so Indian languages offer to the scrutiny of the linguistic student a similar unfinished condition highly favourable for analysis and study.

For the past fifteen years Major Powell and his assistants of the Bureau of Ethnology, with the aid of many collaborators in various parts of the country, have been accumulating vocabularies by means of which to classify Indian languages. The present provisional results of the study of the large amount of material accumulated appear before you on the Linguistic Map, which is coloured to show the areas occupied by the several linguistic families. Of these there appear no fewer than fifty-eight.

What interpretation are we to place upon the astonishing fact that in the territory north of Mexico there were at the time of the discovery fifty-eight distinct Indian linguistic families, containing some 300 or more languages and dialects?

So far as Language is a competent witness, she has exhausted all the evidence thus far accumulated when she has grouped the Indians in fifty-eight families. Back of this point she may not now go except as a theorist and in pure speculation. So far as she is entitled to speak authoritatively, these fifty-eight families are separate entities, which never had any connection with each other. But she recognizes her own limitations too well to dare to state positively that this is the interpretation that must be placed upon the results she has attained. When facts from which to draw deductions fail, men may and do resort to theories. Let us glance at the two broad hypotheses which have been based upon the development theory of language. The first is in effect that all the present languages of the earth are not so unlike that they may not have been developed from a single original parent language. By this view the original language is supposed to have changed and developed into all the various forms of speech that are now spoken or that have ever been spoken. According to this view the families of languages as at present classified have no other significance than as groups of related tongues, the once existing connection of which with other tongues cannot now be proved, because through the process of change the connecting links have been lost.

The second hypothesis assumes that there must have been at least as many original languages as there are now existing families; it assumes, in other words, that the families of speech are fundamentally distinct and therefore cannot have had a common origin. The first theory postulates that from original unity of language has come infinite diversity; the second that the tendency has ever been from original diversity towards unity.

Widely different as are these two theories of the origin of linguistic families, they agree in one essential particular. They both remove the origin so far back in time as to make it practically impossible to *prove* the truth or falsity of either theory.

Both of these hypotheses have able advocates; but for a variety of reasons, which time will not permit me to give, the second is deemed the more plausible. At all events, it best explains many difficulties.

There is abundance of archæologic evidence showing that man has resided on this continent for a very long period, and the character of the remains proves that the farther back in time we go the ruder being he was. Linguistic testimony is to the same effect, and there is no *a priori* reason why man may not have lived upon this continent ages before he learned to talk—no reason, for that matter, why America may not have peopled the earth, if the earth was peopled from a single centre, or why, if there have been several centres of origin for mankind, the Indians, as they themselves believe, may not have originated here where they were found.

It is the fashion, I hardly know why, unless it be the religious bias, for those who hold that language has had but one origin to assume that America is the younger continent, so far as her people are concerned, and to infer that it was peopled from Asia. If America was peopled from Asia in modern times, there should be some evidence of the fact in American languages. But there is no evidence of the sort. None of the American families of language are in any way related to the Asiatic tongues. Bering Strait furnishes, indeed, a perfectly practicable canoe route from Asia to America, but it appears to have been generally overlooked that the Strait furnishes an equally accessible route from America to Asia. The latter is demonstrated by the fact that the Eskimo of Alaska have in recent times sent an Eskimo speaking colony across Bering Strait to Siberia. In other words, so far as direct testimony goes, Asia is indebted to America for a small segment of its people, but America owes no similar debt to Asia. With reference to the origin of our Indian tribes, then, linguistic science is in a position to state this much, that if our Indians came to America, either from Asia or from any other foreign shore, it was at a period so remote as to permit such profound changes in the structure of the language brought here by the immigrants that no traces of genetic connection are now discernible.

If we reject the one origin theory of language, and assume that each linguistic family originated independently, there is obviously not the slightest use of turning to Asia or Europe for anything like a recent importation of the Indian; for have we not fifty-eight distinct origins to account for? Obviously the fifty-eight families are as likely to have originated here as anywhere else; for remember that every country has linguistic families of its own to account for. Is there, then, any possible theory which will meet the case? There is certainly one that is possible, if not probable. It is the theory that, whether born from the soil or an immigrant from other lands, our Indians spread over the entire continent before they acquired organized language, and that from not one but from fifty-eight centres sprang up the germs of speech which have resulted in the different families of language. This theory accords with the idea that there may have been but one origin of man, and that in any event all the Indians from the Arctic to Patagonia are of one race. It does not forbid the supposition that the Indian

was an immigrant from other shores, though it permits the thought that the American Indian may have originated on American soil.

Though this theory seems more probable than the other, which assumes that the languages of our Indians were brought here from foreign shores, it must be frankly admitted that linguistic science is not now and possibly never will be competent to decide between them. If she is unable to decide fully as to the origin of the Indians' languages, how can she be expected to solve the infinitely more complex problem which concerns the ultimate origin of the peoples who spoke them? She certainly has no solution for this problem now. When she considers the number of linguistic families, and the vast length of time it must have taken to develop their languages and dialects, she finds herself confronted by a problem beyond her present powers. And yet the case is not hopeless. Linguistic science is still in its infancy, and its future may contain possibilities far exceeding the dream of the most sanguine. As science has revolutionized the world's processes, and has made the impossibilities of a hundred years ago the common-places of to-day, so like wonders may be achieved in the domain of thought, and the science of Language, with the assistance of her sister sciences, may yet answer the unanswerable questions of the present. . . .

When interrogated as to the origin of the Indian, all that she can now say is that, whether the Indian originated on this continent, where he was found, or elsewhere, it was in bygone ages—ages so far removed from our own time that the interval is to be reckoned, not by the years of chronology, but by the epochs of geologic time; with such problems she affirms that at present she cannot deal.

THE NEW OBSERVATORY IN CATANIA.

WE learn from the *Corriere di Palermo* the following details concerning the new Observatory at Catania, in which active work is about to begin. For many years astronomical studies were carried on with success in the Observatory at Palermo, but owing to the great drawbacks arising from the position of this institution (amongst which the most serious was the close proximity of a railway), it became exceedingly difficult to make precise observations. Some of the astronomers of the Observatory preferred to devote themselves to researches in astronomical physics, in which the inconveniences were less felt, while others who continued to occupy themselves with astronomy of position had to contend against ever-increasing difficulties. The Government, being informed of the state of affairs, nominated in 1888 a Commission to select a more suitable spot, and to make arrangements for a new Observatory; and the only place found suitable was the mountain Consuono, near Bagheria. As an Observatory built there would have the most perfect conditions of quiet and stability, it was decided that it should be devoted to the ancient and classical branch of astronomy of position.

In the meantime, several years ago, a new Observatory had been built on Etna (at a height of 3000 metres), through the activity of Prof. Tacchini, and the approval and help which the project found in Catania. Prof. Tacchini, recognizing the necessity that the Observatory at Etna should be joined with another, that it might work with greater regularity, succeeded in inducing the Government to build and organize the Observatory connected with the University in the town of Catania. This institution is destined principally for the study of astronomical physics, for celestial photography, for meteorology, and for seismology. Direct astronomical and spectroscopical observations will be made for the most part in the upper story of the Observatory with the great refractor, which has an objective of 35 centimetres in aperture and 5½ metres in focal length. It was made by the celebrated Merz at Munich, and the equatorial mounting was executed by the able mechanician Cavignato, of Padua, under the direction of the astronomers Lorenzoni and Abbeti, of that town. Other observations will be made with the equatorial of 15 centimetres in aperture, constructed by the renowned American, Clarke, to which an apparatus can be added for the photographing of stellar spectra. This instrument will be placed in the garden of the Observatory, liberally given by the Municipality of Catania.

In the same place, in a suitable pavilion, will be fixed the photographic telescope, with an objective of 33 centimetres in aperture. This was constructed by Steinheil, and the mounting was made under the direction of Prof. Tacchini in the premises of the clever mechanical engineer, Salmsiraghi, at Milan. With this instrument the Observatory at Catania will take part in the great international enterprise of preparing photographic charts of the stars. In the same garden the apparatus of Huggins will be set up, which serves for the photographing of the solar corona. Besides, at the Observatory on Etna, there is an equatorial, equal to that of the great refractor; and to this it will be possible to apply the same objective of Merz, and another which, for special observations, astronomical and physical, will be taken to the volcano.

Observations in meteorology will be made, some in the upper story of the building, where the apparatus of Mascart for the photographic registration of atmospherical electricity will be worked. Other meteorological observations will be made in the garden; and the Observatory on Etna will be furnished with a collection of meteorological and seismological instruments capable of automatic registration at times when the Observatory will be inaccessible. The seismological instruments will be by degrees removed from the University of Catania, where they are now collected, to a far more suitable place—to the vast subterranean rooms of the Observatory; thus, in the network of stations, this institution will take the first place for the seismological service of Sicily and the adjacent islands.

Prof. Riccò has been appointed regular Professor of Astronomical Physics in the University of Catania, and Director of the two Observatories. He is a member of the International Committee for photographing the stars.

NOTES FROM THE OTAGO UNIVERSITY MUSEUM.

X.—THREE SUGGESTIONS IN BIOLOGICAL TERMINOLOGY.

(1) MR. R. J. HARVEY GIBSON has recently criticized my proposal to use the terms *gamobium* and *blastobium* for the sexual and asexual generations respectively in both plants and animals, and has given some very good reasons for objecting to the latter term.[1] I therefore propose to replace it by *agamobium*, using the word simply as a monomial equivalent of "asexual generation," *gamobium* being similarly employed for the sexual generation. Both terms are equally applicable to plants and animals, and to the various kinds of alternation of generations.

(2) Mr. Gibson, while using the word *ovarium* for the female gonad or ovum-producing organ both in plants and animals, objects to my employment of the anglicized form of it (ovary), because of the confusion arising from the fact that the name is applied to something quite different in Angiosperms. My own impression is that reforms of this kind should be thorough, and that, if it is once admitted that terms of fundamental importance should not be used in one sense for animal and in a totally different sense for vegetable organisms, it would be the ruin of the whole scheme to be obliged to describe the ovarium of an Angiosperm as something contained within an ovule, which is itself enclosed in an ovary. We have already the accurate word megasporangium for the ovule, and I propose to speak of the so-called ovary, by a purely descriptive term, as the *venter* of the pistil.

(3) Every zoologist must acknowledge the services rendered by Haeckel and others in giving names to the more important embryonic stages of animals. It appears to me desirable that the equally important stages in plant development should be similarly emphasized.

In the paper already referred to I have proposed that the one-celled embryo, *i.e.* the immediate product of the conjugation of ovum and sperm should be universally called the *oosperm*, the objectionable name oospore being dropped.[2]

In mosses and vascular plants, as in animals, the next stage of importance after the oosperm is that in which the embryo consists of a mass of undifferentiated or but slightly differentiated cells. It is a matter of minor importance that in some cases a long axis is already obvious, or that in others an apical cell is formed: the stage may be conveniently distinguished by Lan-

[1] See Gibson, Proc. Biol. Soc. Liverpool, vol. iv. (1889) p. 24; *ibid.*, 1887; and Parker, Proc. Austr. Assoc. Adv. Sci., vol. i. (1888).

[2] May I venture to protest against the extended use of the word oosperm sanctioned by my friend Prof. Haddon, who, in his excellent "Embryology," uses it (p. 91) for an advanced mammalian embryo with membranes?

kester's name *polyplast*, which is to be preferred to Haeckel's *morula*.

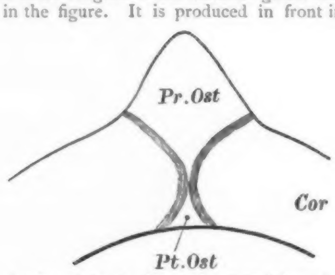
In mosses, the agamobium never gets beyond this stage, the fully formed sporogonium being nothing more than a highly differentiated polyplast. But as in animals the polyplast is succeeded by a gastrula, *i.e.* a stage in which the characteristic organ of animal nutrition has appeared, so in vascular plants it is succeeded by a stage in which the characteristic organs of plant nutrition—the leaf and root—have made their appearance. From ferns up to Angiosperms the first important differentiation after the polyplast is the formation of a cotyledon and of the primary root. This stage, the obvious correlative of the animal gastrula, I propose to call the *phyllula*.

XI.—ON THE PRESENCE OF A STERNUM IN *Notidanus indicus*.

As far as I am aware, nothing answering to a sternum has hitherto been found in fishes, the Urodela being the lowest group in which this constituent of the skeleton is known. In them it consists of a median plate of cartilage formed from paired chondrites (independent cartilaginous elements) developed in the inscriptiones tendineæ immediately caudad of the coracoids, and in close connection with them. In many Anuræ there is found in addition a median element cephalad of the procoracoids. The entire Amphibian sternum is conveniently called by Albrecht the *omosternum*, to distinguish it from the costal sternum of Amniota; the anterior division (omosternum, W. K. Parker) being called the *pre-omosternum*, the posterior division (xiphisternum, auct.) the *post-omosternum*. According to Wiedersheim ("Grundriss," 2te Aufl., p. 58), the phylogeny of the Amphibian sternum is still entirely unknown.

In Elasmobranchs the shoulder-girdle has the form of an inverted arch, usually formed of a single continuous cartilage, due to the union in the middle ventral line of paired elements, one connected with each pectoral fin. In Hexanchus (Notidanus) the right and left sides are described by Hubrecht ("Bronn's Thierreich," Fische, p. 77) as being united by fibrous tissues.

In a skeleton of *Notidanus indicus*, recently prepared for this Museum, the middle region of the shoulder-girdle has the structure shown in the figure. It is produced in front into a blunt



Median ventral region of the shoulder-girdle of *Notidanus indicus*, ventral aspect (natural size). *Cor.*, coracoid region: *Pr.Ost.*, pre-omosternum; *Pt.Ost.*, post-omosternum.

process, while it is evenly curved posteriorly. Two curved areas of fibrous tissue, with their convexities towards the median plane, extend from the anterior to the posterior border, touching one another in the centre, and thus bounding two distinct cartilaginous areas—an anterior (*Pr.Ost.*) of a rhomboid, and a posterior (*Pt.Ost.*) of a triangular form. The anatomical relations of these seem to show that the former is to be considered as a pre-omosternum, the latter as a post-omosternum.

The specimen in question appears to furnish a very good indication of the phylogeny of the sternum, and to show that it arose in the first instance by the segmentation of a mid-ventral element from the shoulder-girdle, in much the same way as the basi-hyal and basi-branchials are formed from the hyoid and branchial arches. T. JEFFERY PARKER.

Dunedin, N.Z., September.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—Prof. M. J. M. Hill, of University College, London, has been approved for the degree of Doctor in Science.

Mr. H. W. Page, M.C., M.B., of Christ's College, has been appointed an additional Examiner in Surgery.

A. H. L. Newstead, B.A., of Christ's College, has been appointed by the Biology Board to occupy the University table in the Laboratory of the Zoological Station at Naples for six months from December 15, 1890.

Mr. J. Y. Buchanan, F.R.S., University Lecturer in Geography, announces a course of lectures in Physical Geography and Climatology for the ensuing Lent and Easter Terms.

The Clothworkers' Exhibition of fifty guineas a year in natural science for non-collegiate students will be awarded next July. Candidates are to apply to the Censor of Non-Collegiate Students.

The General Board of Studies recommend that Mr. R. T. Glazebrook, F.R.S., be appointed Assistant Director of the Cavendish Laboratory. Mr. Glazebrook resigns the Senior Demonstratorship which he has hitherto held.

A very interesting account is published in the *University Reporter* of December 9, 1890, of the course of study in natural science pursued by the University Extension students who were resident in Cambridge during the last Long Vacation. The course included invertebrate palæontology, practical chemistry, and physics. Courses were also given in Greek art, architecture, Egyptology, &c. The Lectures Committee recommend that the experiment be repeated on a larger scale next year.

Additional demonstrators in physiology and in experimental physics are about to be appointed, to be paid out of the laboratory fees of students.

SCIENTIFIC SERIALS.

THE *Quarterly Journal of Microscopical Science* for November 1890 contains:—On the structure of a new genus of Oligochæta (Deodrilus), and on the presence of anal nephridia in Acanthodrilus, by Frank E. Beddard (plates xxxii. and xxxiii.). This large worm, *Deodrilus jacksoni*, measuring thirteen inches in length by nearly half an inch in diameter at the broadest part, was collected by Prof. Moseley in Ceylon; the nephridia in a species believed to be *Acanthodrilus multiporus* are described as connected with the terminal region of the intestine.—Excretory tubules in *Amphioxus lanceolatus*, by F. Ernest Weiss (plates xxxiv. and xxxv.). At the Zoological Station in Naples, the author was able to have a constant and unlimited supply of Amphioxus, and he took the opportunity of experimenting with the view of determining whether the curious patches of modified epithelial cells on the ventral wall of the atrium of Amphioxus had any excretory function, as Johannes Muller had held to be probable; and whether, also, the atrio-cœlomic funnels, first described by Prof. Ray Lankester, had any such function; positive results were not obtained.—Studies in mammalian embryology; ii. The development of the germinal layers of *Sorex vulgaris*, by Prof. A. W. Hubrecht (plates xxxvi. to xlii.). Giving a brief but very lucid sketch of the various views held by recent writers concerning the gastrulation process of the Amniota and the formation of their mesoblast and notochord, placing what is agreed on on the one side, and indicating the points of difference on the other, without any polemical remarks, the author proceeds to an account of the early developmental stages of Sorex, the blastula and the didermic blastocyst, the development of the mesoblast; then follow some theoretical considerations on the gastrulation of the Mammalia, concluding with points of comparison in earlier investigations by other authors.—Terminations of nerves in the nuclei of the epithelial cells of tortoise-shell, by Dr. J. B. Haycraft (plate xliii.). The scutes of the land tortoise (*Testudo græca*), in spite of their hard, dense nature, form a very typical epidermic sensory covering for the animal. As in the soft skin of mammals, the nerves end in localized sensitive spots in the epidermis, and before penetrating this tissue they form a horizontal plexus in the upper part of the connective tissue; figures of the nerve-endings are given.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, November 27.—"The Variations of Electromotive Force of Cells consisting of certain Metals, Platinum, and Nitric Acid." By G. J. Burch and V. H. Veley.

The description of the apparatus, the capillary electrometer,

and the method of working are given fully in the paper. The following conclusions are drawn from the results of the experiments:—

(1) When the metals copper, silver, bismuth, and mercury are introduced into purified nitric acid of different degrees of concentration, and a couple made of platinum, the electromotive force of such a cell increases considerably from an initial value until it reaches a constant and (in most cases) a maximum value. The rise of E.M.F. is attributed to the production of nitrous acid by the decomposition of the nitric acid, and the final value is considered to be due to the former acid only, while the initial value is due for the most part to the latter acid, though it is affected to a remarkable degree by the amount of impurity of nitrous acid, either initially present or produced by minute and unavoidable uncleanliness of the metallic strip and the containing vessel.

(2) If nitrous acid has been previously added to the nitric acid, then the maximum E.M.F. is reached *at once*.

(3) If the conditions—namely, increase of temperature, of impurity, and of concentration of acid—are such as would favour a more rapid solution of the metal, and consequently a more rapid production of nitrous acid, then the rise of E.M.F. is concomitantly more rapid.

(4) Conversely, if the conditions are unfavourable to the production of nitrous acid, the rise of E.M.F. is less rapid.

(5) If any substance, such as urea, be added which would tend to destroy the nitrous acid as fast as it may be formed, then the rise of E.M.F. is extremely slow, being dependent upon the number of molecular impacts of the nitrous acid upon the surface of the metal.

Thus the results obtained by the electrometer and by the chemical balance are in every way confirmatory the one of the other.

The authors propose to conduct further investigations on cells containing other acids, to determine whether the action of them upon metals is conditioned by the presence of their products of electrolysis.

Geological Society, November 26.—Dr. A. Geikie, F.R.S., President, in the chair.—The appointment of Mr. L. Belinfante as Assistant-Secretary was confirmed.—The following communications were read:—Account of an experimental investigation of the law that limits the action of flowing streams, by R. D. Oldham, Deputy Superintendent of the Geological Survey of India. A discussion followed, in which Dr. Blanford, Mr. Binnie, Mr. Topley, and the President took part.—On the rocks of North Devon, by Dr. Henry Hicks, F.R.S. During a recent visit to North Devon the author obtained evidence which has led him to believe that far too little importance has hitherto been assigned to the results of movements in the earth's crust as affecting the succession of the rocks in that area. The supposed continuous upward succession from the rocks on the shore of the Bristol Channel to those in the neighbourhood of Barnstaple, including, according to some authors, no less than ten groups, and classed into three divisions under the names Lower, Middle, and Upper Devonian, is, the author believes, an erroneous interpretation. The beds, he says, have been greatly plicated and faulted, and consequently several times repeated, and instead of being one continuous series, they occur folded in more or less broken troughs. In the Morte Slates, previously considered unfossiliferous, the author found a *Lingula*, and he believes that these slates are the oldest rocks in the area, and formed the floor upon which the Devonian rocks were deposited unconformably. As the result of movements in the earth's crust, the Morte Slates have been brought to the surface and thrust over much newer rocks, producing a deceptive appearance of overlying the latter conformably. The Morte Slates mark the dividing line between the two main troughs. On the north side in ascending order are the Hangman (or Lynton), Combe Martin Bay, and Ilfracombe Beds, and on the south side the Pickwell Down, Baggy Point, and Pilton Beds. Those on the south side of the Morte Slates are, the author believes, a repetition of the beds on the north side. The palæontological evidence is not antagonistic to this view, for an analysis of the Brachiopoda, the only group of fossils in the beds on the south side, which hitherto have been systematically examined, shows that of the twenty species mentioned by Mr. Davidson and others as occurring in the Pickwell Down, Baggy Point, and Pilton Beds (the so-called Upper Devonian rocks), no less than thirteen have already been found in the Middle or Lower Devonian rocks on the north side of the Morte Slates. Four others are recognized Middle Devonian species in other areas; and the three remaining are either doubtful species or ones which have a great vertical range. These facts show that the so-called Upper Devonian rocks in this area do not contain a distinguishing fauna of any importance; and the stratigraphical evidence is opposed to the view that they are a series of rocks distinct from those on the north side of the Morte Slates, which have been classed as Middle and Lower Devonian. In the discussion which followed the reading of this paper the speakers were Mr. T. Roberts, Mr. Marr, Prof. Seeley, Mr. H. B. Woodward, Rev. H. H. Winwood, Rev. G. F. Whidborne, Mr. Hudleston, Prof. Blake, the President, and the author. The President remarked that, in altering the recognized order of succession of these rocks, the author would have to reckon also with continental stratigraphers and palæontologists. Dr. Hicks had not made as clear as might be the evidence for such thrust-planes and faults as his views of the structure of the ground required. The plications and dislocations in South Devon and Cornwall, although abundant and often very complicated, appeared to the speaker to be on a comparatively small scale; and, although he did not wish to insist that this must be the case in North Devon, he felt that it might be so.—A special general meeting was held at 7.45 p.m., before the ordinary general meeting, at which Mr. J. W. Hulke, F.R.S., was elected Foreign Secretary, and Mr. J. J. H. Teall, F.R.S., was elected a Member of Council.

Zoological Society, December 2.—Prof. W. H. Flower, F.R.S., President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of November 1890, and called special attention to the acquisition of a specimen of the Cryptoprocta (*Cryptoprocta ferox*) of Madagascar.—A letter was read from M. A. Milne-Edwards, containing an account of the mode in which the typical specimen of Grévy's Zebra had been mounted for the Gallery of the Museum, and pointing out that the mounted specimen has been carefully modelled after the living animal.—A letter was read from Dr. Emin Pasha, dated "Tabora, East Africa, August 16, 1890," containing an expression of his thanks for having been elected a Corresponding Member; and giving some remarks on the Striped Hyena of that district.—Mr. Richard Crawshay read a paper on the Antelopes of Nyassaland, treating especially of those to be met with west of the Lake. Lichtenstein's Hartebeest was stated to be very generally distributed, and seven other Antelopes to be plentiful. The Kudu, Sable Antelope, and Black-tailed Gnu were seldom met with; but exact localities were given where these Antelopes were to be found. In conclusion, the author added that there are at least two other species of small Antelopes found in the hills, which hitherto be had not been able to identify.—Prof. G. B. Howes read a paper on the peculiar mode of the suspension of the viscera in the Australian Batoid fish *Hypnos subnigrum*.—A second communication from Prof. Howes contained notes on the pectoral fin-skeleton of the Batoidea and of the extinct genus *Squaloraia*, which he maintained must be referred to the Chimæroid group.—Mr. G. A. Boulenger read a paper on the presence of pterygoid teeth in a tailless Batrachian (*Pelobates cultripes*), and added remarks on the localization of the teeth on the palate in the Batrachians and Reptiles.—Mr. H. Seebohm read a paper on the Fijian birds of the genus *Merula*, and gave a description of a new species from Viti-Levu, which he proposed to call *Merula layardi*.

EDINBURGH.

Royal Society, December 1.—Sir Douglas Maclagan, President, in the chair.—This was the first meeting of the Society for the present session.—After the delivery of the President's opening address, Prof. Crum Brown read an obituary notice of Prof. Kolbe.—Dr. John Gibson submitted the results of an analytical examination of manganese nodules, made with special reference to the occurrence of the rarer elements. The nodules which he examined were dredged by the *Challenger* in the North Atlantic. Dr. Gibson has detected the presence of twenty-eight elements. Of these, zinc, tellurium, molydenum, vanadium, and thallium have never previously been observed.—Mr. J. Y. Buchanan, F.R.S., read a paper on the occurrence of sulphur in marine muds and nodules, and its bearing on their modes of formation. The sand and mud at the bottom of the sea, at all depths, forms the food of a numerous and important

ground-fauna. In the search after their food, the animals are continually passing large quantities of the mud through their bodies. Evidence of this is supplied by the coprolitic structure of all finer muds, which is rendered visible by careful levigation. The effect of trituration of silicates, under distilled water alone, has been shown to result in partial decomposition. When the milling apparatus is situated in the inside of an animal, and is therefore moistened by organic secretion as well as by sea-water, it is well-known that reaction, resulting in the reduction of the sulphates in the water, takes place. If the silicates, which are being triturated and partially decomposed, contain iron and manganese, the sulphides of iron and manganese are produced, and, being rejected by the animal, form a more or less bluish mud. When this mud lies on the surface of the sea bottom, and is exposed to the action of the sea-water, which always contains dissolved oxygen, the sulphides are oxidized, so that the reddish-brown surface layer, so frequently observed, is formed. When the sulphides come into contact with pre-existing higher oxides, such as Fe_2O_3, ferrous oxide is formed, and sulphur is separated. Reasoning on these grounds, Mr. Buchanan concluded that it must be possible to find free sulphur in all marine muds. In blue muds the sulphur is partly pre-existent, and is partly formed on drying in the air. In red clays, and in manganese nodules, it is wholly pre-existent. Examination of twenty-five muds, and of nodules from all regions, comfirmed this view, as they all gave up sulphur on exhaustion with chloroform. Mr. Buchanan made a number of experiments on the reaction of the sulphides of iron and of manganese on the ferric salts and oxide. He found that precipitated MnS acts on solutions of iron salts exactly in the same way as an alkaline sulphide does. It reduces ferric salts, and at once, and completely, precipitates ferrous salts as FeS. The reactions of MnS on ferric hydrates gave interesting indications regarding the probable nature of the "red cherty particles" so frequently observed in manganiferous bottoms. Mr. Buchanan concludes that the ochreous matter, hydrated oxides of iron and manganese, so abundant on the surface of the bed of the ocean, spring from silicates, and other combinations, by reduction to sulphides in a process of digestion and subsequent oxidation by the atmospheric oxygen of the sea-water. They may also be re-reduced and re-oxidized repeatedly.—Dr. John Murray communicated an anatomical description, by Mr. F. E. Beddard, of two new genera of aquatic Oligochætæ.—Mr. John Aitken exhibited and described a pocket form of his dust-counter, intended for meteorological observations. This form of the instrument is much reduced in size, and is greatly simplified in construction.

PARIS.

Academy of Sciences, December 1.—M. Duchartre in the chair.—On the Fourchambault tornado, by M. H. Faye. The author points out, among other things, that it will be extremely interesting to verify M. Dommet-Adamson's assertion that the gyrations took place in the same direction as the hands of a watch. This has also been noticed in some tornados in the United States, but is very rare.—On some new fossil plants observed at Portugal, and indicating the passage from the Jurassic to the Lower Cretaceous formation, by M. G. de Saporta.—Observations of Zona's new comet, made at Algiers Observatory, with the *coudé* equatorial of 0·318 metres aperture, by MM. Trépied, Rambaud, and Renaux. Observations for position were made on November 17, 18, and 20.—On a new method of displacement of a double cone, by M. A. Mannheim.—On the compressibility of mixtures of air and carbon dioxide, by M. Ulysse Lala. The author has experimented upon mixtures containing 11, 19·35, 26·29, 33·33, 40·08, 47·54, and 56·92 per cent. of carbon dioxide. Between the limits of his experimental pressures, viz. 100·38 cm. of mercury, with volume = 1, and 1613·96 with volume = 0·5, the compressibility of mixtures of dry air and carbon dioxide, when the quantity of the latter gas does not exceed about 22 per cent., is contained between those of the two gases used. It was observed that with mixtures richer in carbon dioxide the compressibility increased.—Reflection and refraction by bodies with abnormal dispersion, by M. R. Salvador Bloch.—On a new process for the differentiation of arsenic and antimony metallic mirrors produced on porcelain, by M. G. Denigès.—On an epithelial fibrillous tissue of Annelides, by M. Et. Jourdan.—Influence of acetic acid on gaseous respiratory changes, by M. Alfred Mallèvre.—On a new haemato-alkalimetric method, and on the comparative alkalinity of the blood of the vertebrata, by M. René Drouin. It is shown that if the alkalinity of the serum of the blood of different species of vertebrata be tabulated, the values may be divided into groups following precisely the same order as the zoological affinities, and that the order in which these classes succeed one another is the same as that of the increase of activity of respiration.—On the structure of nervous centres of *Limulus polyphemus*.—On the external differences presented by the *Nematobothrium, a propos* a new species (*N. Guernei*), by M. R. Moniez.—The enterocele nervous system of Echinodermata, by M. L. Cuénot.—Experimental researches on the locomotion of Arthropods, by M. Jean Demoor.—Comparative influence of light and weight on mosses, by M. Eugène Bastit.—On the presence of lactifers in Fumariaces, by M. L. J. Léger.—Principal indices of refraction of anorthite, by MM. A. Michel Lévy and A. Lacroix.—On the tempest of November 23 and 24, 1890, and the vertical movements of the atmosphere, by M. Alfred Angot.

STOCKHOLM.

Royal Academy of Sciences, November 12.—On the spectrum of absorption of brome, by Prof. Hasselberg.—Determinations of the longitude between the Observatories of Stockholm, Copenhagen, and Christiania, executed by C. F. Fearnley, F. C. Schjellerup, and D. G. Lindhagen.—On the recently executed expedition to Spitzbergen, conducted by G. Nordenskiöld, and its scientific results, by Baron Nordenskiöld.—On the results of the latest Swedish hydrographic expedition in Skagerack and Kattegat during the winter 1890, by Prof. O. Pettersson.—On floating pumices and slags stranded on the shores of North Europe, by Hr. H. Bäckström.—Mineralogical notes, iii., by Hr. G. Flink.—On the employment of infinite determinants in the theory of linear, homogeneous, differential equations, ii., by Hr. H. von Koch.—On a single case of permanent movement with rotation, by Hr. E. Phragmén.—On the waterspout at Wimmerby, July 4, 1890, by Hr. T. Wigert.—On the oxidation of phenyl-methyl-triazol-carbon acid, ii. Phenyl-triazol-dicarbon acid ; *a*. The constitution of phenyl-triazol-carbon acid, by Dr. Bladin.—On the constitution of the cummenyl-propion acid, by Prof. Widman.—On the shifting from propyl to isopropyl in the cumin series of the latter, by Prof. Widman.—A review of the mosses of Småland, by Hr. R. Tolf.—Researches on the soft parts of the supernumerary radii of the hand and the foot, by Miss A. Carlsson.

CONTENTS.